PALESTINE EXPLORATION QUARTERLY

Embodying the *Quarterly Statement* of the Palestine Exploration Fund and the *Bulletin* of the British School of Archæology in Jerusalem

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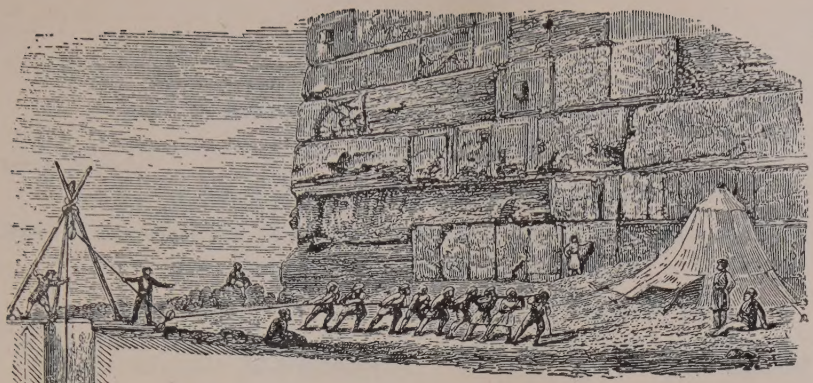

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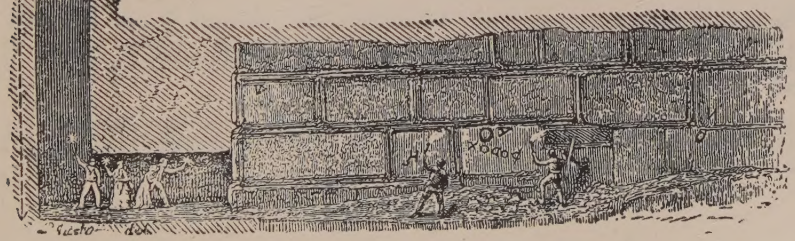

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(All photographs by N. LORD, 1954)

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THE PALESTINE EXPLORATION FUND

NOTES AND NEWS

The most exciting news comes from Jericho. While the Arabs and the Israelites renew the ancient feud between Jacob and Esau, Dr. Kenyon has been steadily digging her way back into a past far more ancient than either. The report of her last season's work, contained in the present issue of the journal, raises new problems and threatens the foundations of long-held archaeological views as to the origin and antiquity of civilization itself. It has been almost, one might say, an article of faith that the earliest civilizations known to us had their birth in the two great river-valleys of the Nile and the Tigris-Euphrates. Now, in Miss Kenyon's words, " The evidence from Jericho of the antiquity, complexity and long development of the pre-pottery Neolithic culture is completely revolutionary for all established conceptions of the beginnings of settled life ". The evidence referred to by Miss Kenyon and contained in her report in the present issue of this journal, would seem to prove the existence of a civilization at Jericho in a period not long after the end of the Pleistocene: that is to say, to a point of time about three thousand years earlier than the beginnings of civilization in Egypt or Sumer. A further shock for archaeologists comes from that eminent archaeologist, Sir Mortimer Wheeler: commenting on Miss Kenyon's startling discoveries in an article in the current issue of *Antiquity*, he puts forward the theory that the beginnings of civilization should not be looked for in river-valleys subject to seasonal destructive flooding, but in oases, of which Jericho with its perennial spring is a notable example. Moreover, Miss Kenyon points out that the remarkable pre-pottery Neolithic fortifications indicate the existence of enemies who must have reached a sufficiently advanced stage of civilization to be able to attack these fortifications with success and she suggests that centres of such a civilization may possibly be sought in the Jordan valley, where there are many unexplored ancient sites. So here are thrills galore and matter to keep archaeologists busy for many a day testing and revising their foundations. Meanwhile, we hope that Miss Kenyon will obtain all the necessary financial support needed to complete the task she has so skilfully and successfully begun. We also trust that, in spite of these difficult times, readers of the PEQ who can afford it will contribute what they can to this most significant enterprise.

In *Syria*, Vol. XXXII, fasc. 3–4, the place of honour is given to M. Parrot's report on the 10th season of excavations at Mari. The most important result of this campaign was, in M. Parrot's words, " the re-appearance of the Akkadians at Mari." The discovery of three bronze vases proved by the inscriptions which they bore that Naram-Sin, the grandson of Sargon I, had conquered Mari, and had established one of his daughters there as a priestess of Shamash. This was a discovery of no small chronological importance. One of the bronzes mentioned the name of a second daughter of Naram-Sin, but the state of the inscription did not apparently permit of the definition of her position. It is interesting to note that, as M. Parrot points out, Naram-Sin had also installed his daughter Enmenana as high priestess of Nannar at Ur. The same number of *Syria* also contains an interesting article by R. P. Pelletier in which he discusses the question whether Antiochus Epiphanes had dedicated the veil of the Temple at Jerusalem to Zeus in his temple at Olympia.

Much interest is being taken in that document from Qumran which has been called " Hymns of Praise ", the name given to it by its first editor, the late Professor Sukenik. In the *Revue Biblique* for April, 1956, there is a valuable article on this document by H. Bardke. The author believes that the Hymns were composed by a single individual for use in private devotions, somewhat after the *Spiritual Exercises* of Ignatius Loyola. He discusses the possibility that they may have been written by the Teacher of Righteousness himself, but wisely refuses to be dogmatic on the point. The restraint with which the author discusses the problems of the *Hymns* is in laudable contrast to the rash dogmatism which has characterized some other approaches to these interesting documents.

In the *P.E.Q.* for October, 1954, an account was given of Dr. Kuhn's theory of the contents of the copper scrolls which were discovered in cave 3Q at Qumran. His theory was that the Qumran community was a wealthy one and that, foreseeing the disasters which would follow the Bar Cochba revolt, they had prepared an inventory of their valuable possessions with indications of the places where they had been hidden. Now Dr. Kuhn's inspired guess has been vindicated. Professor Wright Baker has successfully accomplished the delicate task of " slicing " the corroded copper sheets, making it possible to decipher the contents. They are, apparently, an inventory of a large number of buried hoards of treasures of gold and silver of immense value. The treasures are listed as being buried in various hiding-places in an area extending from Hebron to the neighbourhood of Nablus. One can imagine that a feverish treasure-hunt will soon be on foot in Israel, to whom such treasures would be of inestimable value in their present financial straitness.

Mr. Seton Lloyd's excavations at Beycesultan have already been referred to in this journal. Now Mr. R. D. Barnett, of the British Museum, has described in *The Times* of August 31st of this year some of the significant results of the third season of excavations at that site. One of the most important discoveries was that of a clay seal of the button type bearing a Hittite hieroglyphic inscription. It has been dated at a period prior to 1900 B.C. Hence it is the earliest example of any kind of writing, hieroglyphic or otherwise, found in Anatolia. It confirms the view maintained by Mr. Seton Lloyd in *The Times* of November last year that there were Indo-Europeans in south-west Anatolia as early as the end of the Early Bronze Age. Nor is this the only important inference to be drawn from the archaeological evidence from Beycesultan. Mr. Lloyd has suggested that Beycesultan was the capital of the Luvian-speaking inhabitants of Arzawa, possibly destroyed by Labarnaš I, the founder of the Old Hittite kingdom. Still more interesting is the suggestion of cultural links between Arzawa and Crete. Mr. Barnett concludes his valuable article by saying " But it appears plain that it was at least from Anatolia that the Minoans brought their architects, and this is no small point to have established. It may well be that the Minoan population contained important elements which were allied to the pre-Hittite, pre-Luvian people of Anatolia ".

In the third number of the new *Journal of Semitic Studies*, which has made such an auspicious start, there is an interesting article by a Scandinavian scholar, Bo Reicke, entitled " The Knowledge Hidden in the Tree of Paradise ". There is also an important article by Professor Ullendorff on Hebraic-Jewish elements in Abyssinian Christianity. There are also a number of valuable reviews.

In the *B.A.S.O.R.* for April, 1956, Professor Nelson Glueck reports the results of his fourth season of exploration in the Negeb. In his own words, " The *tabula rasa* of the Negeb is being rapidly filled in ". One important result has been to establish the existence of extensive sedentary settlement in the Negeb during Iron Age II and, to a lesser extent, during Iron Age I. Dr. Glueck points out that the assumed lack of sedentary occupation in large parts of the Negeb over long periods of history came from lack of knowledge.

The Editor has to announce that the present issue of the *Quarterly* is the last for which he will be responsible. After twenty-five years, during which the journal has seen many changes in format, content and personnel, the editorship will now pass into the very competent and scholarly direction of Professor F. F. Bruce, the head of the Department of Biblical Studies in Sheffield University. All material intended for submission with a view to

publication should be sent direct to him at 79, Bower Road, Sheffield 10. The Editor, in taking leave of readers and contributors, would like to thank all concerned for much kindness and consideration extended to him during the years of his tenure and to express the hope that similar kindness and consideration may be shown to his successor.

OBITUARY

COL. S. F. NEWCOMBE, D.S.O.

The death of Colonel Stewart Newcombe removes another distinguished name from the Executive Committee of the Palestine Exploration Fund. If he had lived in the time of King David he would certainly have found a high place among David's " mighty men ". His exploits in Arabia and Palestine during the first World War are well known and are worthy to be ranked with those of his more celebrated companion in arms, T. E. Lawrence. In the early part of 1914 he carried out a survey of the southern border of Palestine across the Sinai Peninsula to Beersheba, under the auspices of the Palestine Exploration Society, and prolonged the original survey to the Egyptian frontier. After a long and distinguished career, he retired from the Army in 1932 with many honours. He took a lively interest in the welfare of ex-service men and went out to Australia to organize settlements for them in that continent. With his intimate knowledge of Palestine and the Middle East, he was a most valuable member of the Executive Committee of the PEF and served for several years as Hon. Treasurer. His friend, Capt. Liddell Hart, has well summed up his character as a soldier, " While Newcombe's war career provided an outstanding demonstration of the power of audacity, it was accompanied by a degree of sagacity uncommon in men of such adventurous spirit. His balance and fairness of judgment were as impressive as his exceptional blend of moral and physical courage ".

PROFESSOR J. GARSTANG

Professor John Garstang has served his generation well as a scholar and as a distinguished field archaeologist. He was a mathematical scholar of Jesus College, Oxford, but turned his attention to archaeology while still an undergraduate. At the age of twenty-six he was appointed honorary Reader in Egyptian archaeology at Liverpool University and, five years later, he became Professor of the Methods and Practice of Archaeology at the same University, a post which he held until his retirement in 1941. During those years he was extremely active and acquired a wide range of experience in field archaeology in Egypt, Nubia, Asia Minor and Palestine. He was an early authority on Hittite civilization and his book, *The Hittite Empire*, is still a standard work on that subject. During the First World

War he served in France as a Red Cross delegate and was awarded the ribbon of the Legion of Honour. In 1920 he became director of the British School of Archaeology in Jerusalem and first director of the Department of Antiquities of the Government of Palestine, an institution which was largely his own creation. His name is perhaps most widely known in connection with his excavation of Jericho, which covered several years. It was specially notable for the view which he propounded, based on his interpretation of the archaeological evidence that the Israelite capture of that city under Joshua took place about 1400 B.C., thereby upsetting the commonly accepted date of the Exodus. Later work at Jericho has thrown doubt on this view but there can be no doubt as to the value of his work at Jericho. His last and most important work was done as director of the Neilson Expedition to the Near East, in which capacity he conducted the excavation of the Cilician site of Mersin. He was appointed the first director of the British Institute of Archaeology in Ankara when it was opened in 1948. He was made a C.B.E. in 1949. He was for many years a valued member of the Executive Committee of the Palestine Exploration Fund.

Correction :

On page 59 of the *P.E.Q.* for January 1956 item 4 should read : *Excavations at Ramat Rahel*, pamphlet published by the Government printer for the Ministry of Education & Culture Department of Antiquities, Jerusalem.

EXCAVATIONS AT JERICHO 1956

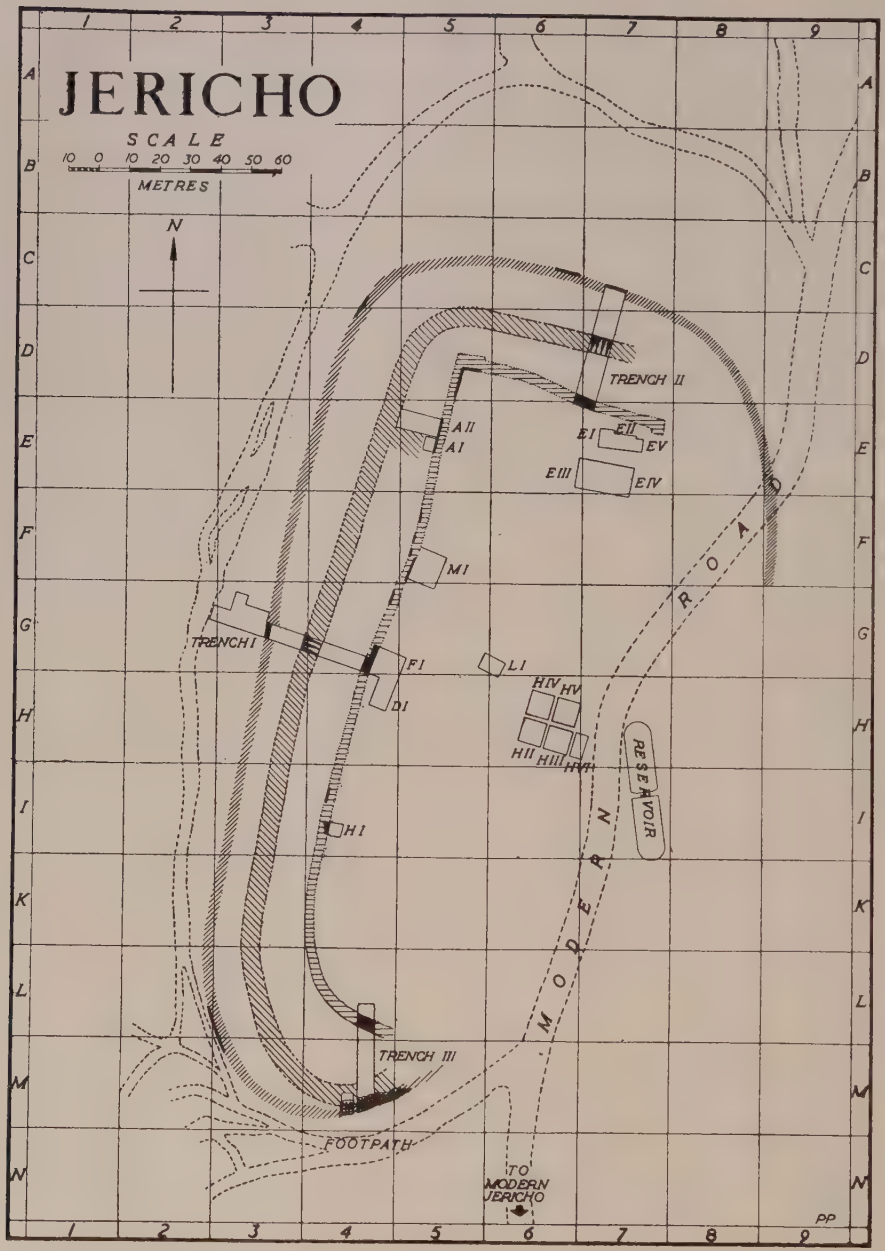
By Kathleen M. Kenyon

Director, British School of Archaeology in Jerusalem

The start of the 1956 season at Jericho was delayed nearly a month by the political troubles in Jordan. During this period, members of the expedition received most kind hospitality from the American School in Jerusalem, and for part of the time some of the party were guests of members of the British community in Damascus, through arrangements made by Mr. Oliver Myers. I would like here to record publicly our thanks for all this kindness.

Work at Jericho began at the end of January, and was continued until the middle of April. The results were of such outstanding importance that the season, in spite of its curtailment, may be considered the most successful to date. The expedition once more received a warm welcome and every assistance from the Department of Antiquities and its Director, Mr. G. L. Harding. Much help with tents and other equipment was given by the Arab Legion. To these organs of the Hashemite Kingdom of Jordan and other Government bodies, our grateful thanks must be expressed.

The United Kingdom side of the expedition was once more sponsored by the British School of Archaeology in Jerusalem, the Palestine Exploration Fund and the British Academy. Major contributors towards the cost were Sydney University, the Russell Trust, the Ashmolean Museum, Birmingham City Museum, the Australian Institute of Archaeology and Emory University, Georgia, and a large number of other museums and universities also made generous grants. This year, our collaborator in the field was the Royal Ontario Museum, Toronto, whose part of the expedition was financed by the *Toronto Globe and Mail*. Dr. A. D. Tushingham, Director of the Archaeology Division of the Museum, acted as Assistant Director of the Expedition, a position which he had filled in 1952 and 1953 as representative of the American School of Oriental Research. A reporter of the *Globe and Mail* was a member of the party, and frequent articles on the progress of the work appeared in the paper. Toronto also provided two other members of the Expedition. The American School of Oriental Research in Jerusalem was not this year a formal collaborator, but contributed generously, and several members of the School took part in the excavations. An important contribution to the work was made by the Deutsche Forschungsgemeinschaft, which enabled Dr. Gottfried Kurth and two assistants to study the human skeletal remains on the spot. The total membership of the party was twenty-


JERICHO
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RESERVOIR
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FOOTPATH
TO MODERN JERICHO
MODERN ROAD
PP

Fig. 1

six[1]; the successful results of the season's work are due to the excellent work carried out by all.

At the end of February, we received a visit from a Television Unit of the B.B.C., led by Paul Johnstone, to take a film for a programme produced in July in the Buried Treasure series. Shots were taken of all aspects of the work, and it is generally agreed that the result was a great success. The filming also provided the occasion for a very welcome visit from Sir Mortimer Wheeler, Chairman of the British School.

Pre-Pottery Neolithic Period

In three of the areas being excavated (see plan, fig. 1), Trench I with Squares D I and F I, now combined with it, Square M I and Squares E I, II and V, clearance had already penetrated deep into the pre-pottery Neolithic levels in 1955, and had proved that there was a long succession of levels of this phase. At the south end of the tell, in Trench III, levels had also been touched at the extreme south end of the trench; this year, though the main excavation did not reach the pre-pottery levels, clearance of the pottery-Neolithic pits, showed that these pits cut into a considerable sequence of the plastered floors typical of the pre-pottery phase. In the northern trench, II, again excavation has not yet reached these levels. But the inclusion in the make-up of the bank beneath the Middle Bronze Age plastered scarp of many objects, such as stone bowls, typical of the pre-pottery levels, gives a strong presumption that these levels must have extended to the north of the line of the Middle Bronze Age defences, for the evidence of Trench I is clear that the bank was composed of soil obtained by the clearance of all earlier levels, down to bed-rock, outside the defences. The extent of the pre-pottery Neolithic town had thus been established, with a minimum area of 7.8 acres.

In Trench I and the adjoining Squares D I and F I, clearance beneath the line of the Early Bronze Age city walls on the western crest of the mound had revealed three successive defensive walls belonging to the pre-pottery Neolithic, following almost exactly the line of their Bronze Age successors. The most imposing of these was the earliest, founded on bed-rock and preserved to a height of 4·25 metres. In 1955, this was found to have at its rear an imposing tower, solidly built of stones.

[1] Miss K. M. Kenyon, Director; Dr. A. D. Tushingham (Toronto), Assistant Director; Mr. C. Burney, Mr. B. Power (Toronto), Dr. H. Franken (Leiden), Mr. M. Lovelace (A.S.O.R.), Mr. G. Mindenhall (A.S.O.R.), Dr. S. Holm-Nielson (Denmark), Mr. D. Liversage (Trinity College, Dublin), Miss P. Oliver, Mr. R. Cleveland (A.S.O.R.), Site Supervisors; Lady Wheeler, Tombs Supervisor; Mr. J. Carswell, Tombs Draftsman; Mr. P. Pedrette, Surveyor; Mr. P. Dorrell, Photographer; Miss D. Marshall, Registrar of finds, Miss L. Witherall and Miss A. Battershill, Technical Assistants; Miss W. Needler (Toronto), and Miss L. Pearre, Assistant Registrars; Miss P. Wolfenden, Assistant to Surveyor; Dr. G. Kurth, Dr. G. Nobis and Dr. B. Kummer, Anthropologists; Mr. D. Spurgeon (Toronto) Reporter; Miss E. Callow, Housekeeper.

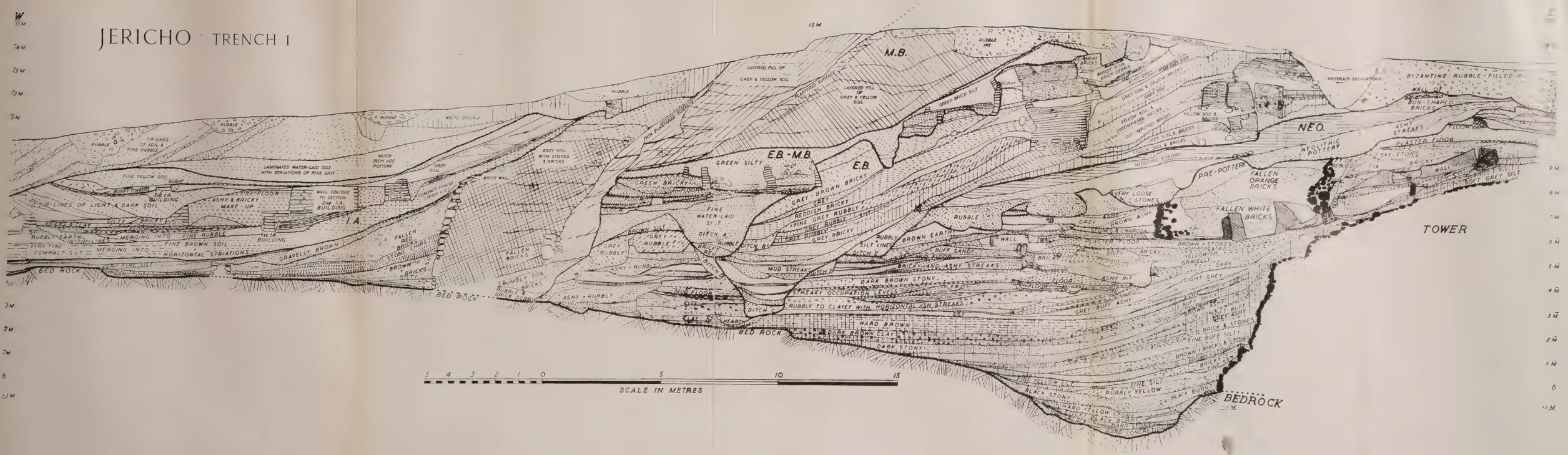
The 1956 excavations completed the clearance of Trench I to bed-rock throughout its length (section fig. 2), and have revealed new features in the defences at this point. Outside the wall there proved to be a ditch, 8·50 metres wide and 2·10 metres deep, cut out of the solid rock. The labour of cutting this ditch must have been enormous. There were no obvious pick-marks, and indeed one of the features of the flint industry is the lack of heavy picks or chisels. The ditch must have been excavated by cracking and crushing the rock with stone mauls, perhaps assisted by the time-honoured primitive method of the use of water and fire. The result is a most impressive adjunct to the great wall (pl. VI).

The interest of the tower was also much increased by further discoveries. The removal of a stratification baulk across the centre revealed a gap in the stone packing, outlined by large slabs of stone. This proved to be the entrance of a stairway descending into the heart of the tower (pl. VII and pl. VIII). Twenty-eight steps were in position, descending at an angle of 30° to a depth of twenty feet (fig. 3 and pl. VIII. 2). Each step was formed of a single slab of stone, hammer-dressed to a smooth finish, and the obliquely-sloping roof was built of similar slabs, some of which were at least a metre by a metre and a half across. The walls of the stairway were slightly concave in elevation, and were covered with mud-plaster, which clearly showed the finger-marks of the ancient plasterers. At its foot, the stairway gave onto a horizontal passage running eastwards. This was cleared for a length of 4 metres, and probably there was here an opening to the exterior of the tower. But, unfortunately, at this point the edge of the tower lies just beyond the excavated area, and the excavation of fifty feet of the tell, together with twenty feet of our own dump will be necessary to uncover the opening of the passage.

The planning and construction of such a monumental piece of architecture at this very early period is nothing short of amazing. Its purpose for the moment can only be conjectured. It is tempting to see in it a resemblance to the great water-shafts of Jerusalem, Gezer and Megiddo, belonging to a much later period. The spring at Jericho was undoubtedly of prime importance to the life of the community, and it is possible that this is the solution. Alternatively, the passage and stairway may simply have been to enable the top of the tower to be manned, which would make them part of a remarkable piece of military architecture. It is hoped that it will be possible to clear the outside of the tower next year and throw light upon these problems.

At a stage when the passage had silted up to within 60 cm. of the roof, some eleven bodies were inserted in it, almost inextricably jammed in. It is clear that use as a burial chamber was not the original purpose, since there was the preceding long period during which the silt had accumulated. It is tempting to see in the skeletons the bodies of the last-ditch defenders of the tower. But by that time, the passage had ceased to be an effective means

JERICHO TRENCH I



of access to the summit, and the probable interpretation is that it was a convenient place in which to dispose of bodies, after the tower had gone out of use.

The stratigraphical position had already made it clear that this great defensive system belonged far back in the history of Jericho. But this year it was established that there were yet earlier phases. The tower shows three distinct building stages, two successive additional skins of stone walls being added to an original core (see *P.E.Q.* 1955, pl. XIII. 2). The first skin is apparently a continuous one, encircling the whole tower. The outermost is partial only, and belongs to a curious set of structures, curving in plan and often in elevation, with plastered walls, but no doorways. They must therefore have been cellars or storage bins. It is to this stage that the great stone wall belongs, since it was clearly built against this outer skin (pl. IX. 1).

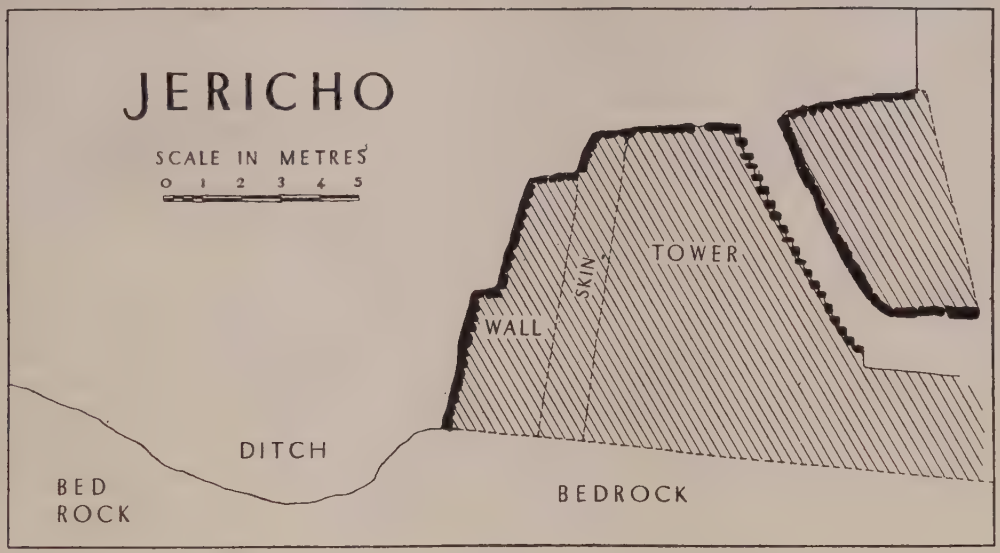


Fig. 3.

The tower was therefore in existence before this wall. The clearance of the adjoining area is not yet complete, but a portion has been exposed of what is almost certainly an earlier town wall (pl. IX. 2). This is a massive structure, surviving to a height of 3·50 metres, and apparently, unlike its successors, free-standing on the inner as well as the outer side. This wall was broken through by the inner skin added to the tower, and therefore presumably goes with the original core. The wall going with the second stage, that of the inner skin, may be represented by the lower courses of the later wall, which are of a noticeably rougher build and at a different inclination. When the later wall, contemporary with the outer skin, was built, the space between it and the first wall was packed with rock-chippings.

These were almost certainly derived from the cutting of the ditch, which would thus be contemporary with the third stage.

The history of this defensive system is thus a long one. Clearance is not yet sufficiently far advanced to say what precedes it, whether any earlier defensive system or a period of undefended settlement. The inner skin of the tower does not rest on bed-rock; its base has so far been cleared in one small area, where there are at least two floors between the bottom of the wall and bed-rock. The base of the original core has not yet been exposed, for it is difficult to do so without destroying the inner skin, which would be regrettable, but may be essential.

The excavation of these massive defences, and the discovery of their complexity, has been the most important structural find of the past season. But another find has been perhaps even more important as regards the history of the site. This was the discovery that there were two quite distinct pre-pottery Neolithic phases. The previous four years' work had made us familiar with the characteristics of the culture as so far revealed. The houses were of an almost stereotyped plan, the chief features of which were rectangular rooms with wide entrances through doorways in rounded nibs, walls constructed of thumb-impressed bricks of a flattened-cigar shape, and the walls and floors of all principal rooms covered with a highly burnished plaster. The flint industry, the stone bowls, the querns and the other utensils and implements had become entirely familiar.

In the debris overlying the face of the great town wall we had in 1954 found bricks of a different type. This year we found them in position. They are an elongated oval in plan, of different varieties of a plano-convex type, and from the pointed section of the most characteristic form, the name " hog-backed " has been given to them (pl. XI. 2). The buildings of which they form part are entirely different from those hitherto found. The walls seem all to be curved, and the rooms approach the circular in plan. They are apparently sunk beneath the surrounding level, and are approached by sloping entrance passages (pl. X. 1.) In one case, there were certainly steps with wooden treads leading down into the room (pl. X. 2). The walls tend to incline inwards as they rise, suggesting that the rooms were domed, and there are frequent traces of timber or wattling in the walls, suggesting that the roofs may have been of plastered branches. In one case, the face of the wall was composed of mud-covered reeds or bamboos. So far, no complete plan of a house has been excavated, so we do not know if they were single-roomed structures or consisted of a number of these curvilinear rooms. The floors were of beaten mud, and not of burnished plaster. The equipment of the inhabitants of these buildings was noticeably different from that of their successors. The flint industry has not yet been studied in detail, but a first inspection suggests that it has little in common with that so far found. The finely worked stone bowls are missing, as are the characteristic querns. One of the most striking points is a rich and finely worked bone industry.

The differentiation of the two phases is thus very marked. It is premature to apply sequence numbers to them, and for the time being it is convenient to refer to the later phases as the plastered-floor stage and the earlier as the hog-backed brick phase.

Buildings of this new type have now been found in Squares E I, II, V, Square M I and Squares D I + F I (see plan, fig. 1). Occupation of this period therefore covers at least half the Tell. Its separation from the plastered-floor phase was most conveniently emphasised in Site E. Running the length of the north side of the three squares was found to be a stream-bed, probably of storm water (pl. XI. 1). This had cut deeply into the houses of the hog-backed brick phases, and evidence of at least three cuttings-down and then silting up indicates an appreciable passage of time, though whether of three years, or ten, or even fifty, it is impossible to decide. Immediately above the final silting-up appears the first plastered-floor house (section, fig. 4). Thus between the two phases there is not only a break in culture, but a marked stratigraphic break, indicating a period of decay or even abandonment.

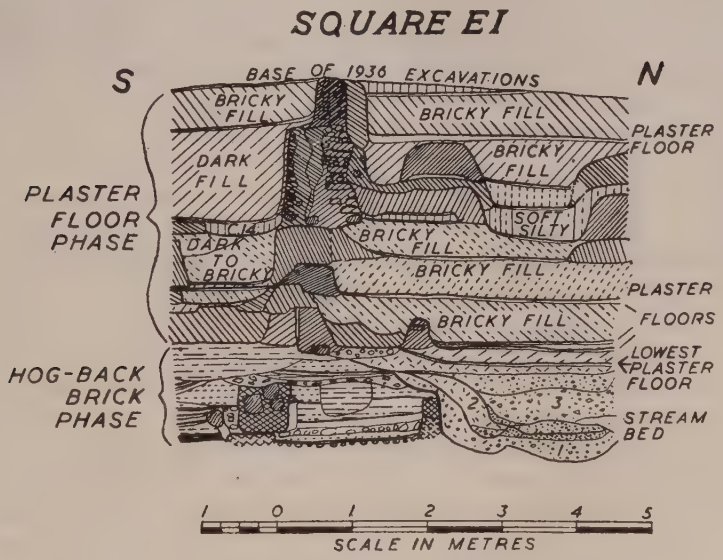


Fig. 4.

This break corresponds to a layer of soft silt over the tower in Squares D I + F I, and to the silting-up against the face of the great wall. For one of the most important results of the establishment of the two cultures was to show that the tower and the defences associated with it belonged to the earlier phase. The buildings associated with the tower are built of hog-backed bricks. The first buildings above the layer of debris which covers it are of the plastered-floor type.

The evidence of the 1956 excavations therefore carries back the history of Jericho as a town to a remote antiquity on the evidence of stratigraphy alone. It is now possible to provide the beginnings of an absolute dating, for the first Carbon-14 datings are now available. These datings are based on the surviving radio-activity of organic matter. All organic matter absorbs radio-activity when it is alive and loses it when it is dead (e.g. when timber is cut down, or shells are no longer part of living organisms) at a rate which has been established. Therefore, a measurement of the surviving radio-activity in organic matter such as charcoal, recovered from excavations, gives a date for the levels in which it is found. On present methods, there is an appreciable margin of exactitude, and there are many possible sources of error or contamination, as scientists who work in this field fully recognise. But within these limitations, this method of dating does give us a guide towards an absolute date which is otherwise impossible for the periods before reference to an established calendar can be established, that is to say, roughly before the year 3000 B.C. The Jericho datings based on material obtained in the earlier seasons' work, concern only the plastered-floor phase. The results are not yet entirely consistent, for methods employed in different laboratories have yet to be correlated[1]. But the *youngest* date given for a level marked C.14 on fig. 4. in Square E I is c. 6250 B.C. and for a level marked C.14 on fig. 1 in Square F I the corresponding date is c.5850 B.C. Both these levels are well advanced in the plastered floor phase. In Square E I, the level is the sixth of seventeen main phases surviving below the level reached by the 1935–6 excavations; in Square F I, it is the thirteenth of twenty-one phases of the plastered-floor phase (on the actual section on fig. 2, a number of phases established in adjoining areas are missing owing to terracing and denudation). Therefore, before these dates in the seventh millennium lie a long period of the plastered-floor phase and the whole of the hog-backed brick phase, the complexities and duration of which have been indicated above.

Before the general significance of these facts is discussed, some other interesting finds must be reported. In the first place, two more of the human skulls with plastered features of the type found in 1953 were discovered. They were in exactly the same stratigraphical position as the earlier finds, in fact in the ruins of the same house, at a distance of about 6·75 metres from the first group. They are unfortunately not in such good condition. The first to be found was observed in the side of the Square where storm water had cut a gully, and as a result the front of the face was destroyed. The second lay behind it (pl. XII. 1), and though it is intact, earth pressure has forced the upper part of the cranium onto the lower, and the eyes are almost hidden by the plaster of the cheeks. One very interesting feature, however,

[1] Some of the results have been obtained in the laboratory of the Royal Institution, working in connection with the Institute of Archaeology under the direction of Professor F. E. Zeuner, and some at the University of Groningen. To these institutions and especially to Professor Zeuner, the Jericho Expedition is deeply indebted.

is that there are traces of a painted moustache (pl. XII. 2). This is additional evidence of the individuality of the different heads, for the others were clean-shaven.

The second find established the fact that the hog-backed brick people were also interested in skulls. Two groups, belonging to this phase, of skulls detached from bodies, were found, in Square E II and Square D I. In the first (pl. XIII. 1), the skulls are arranged in a neat circle, facing towards the centre. In the second, they are arranged in rows of three groups of three, all facing in the same direction (pl. XIII. 2). In another instance, a number of infants' skulls and one intact infant burial were placed beneath the foundations of a curious bath-like structure, in circumstances which strongly suggest sacrifice. It is hoped that further excavation will throw light on the ceremonial practices of this earlier pre-pottery Neolithic group.

The evidence from Jericho of the antiquity, complexity and long development of the pre-pottery Neolithic culture is completely revolutionary for all established conceptions of the beginnings of settled life. Hitherto, a scheme with sites such as Byblos, Mersin and Hassuna providing evidence of the first stages of village life in the middle of the fifth millennium, with perhaps Jarmo showing a first, pre-pottery, stage about 4750 B.C., had seemed to provide a reasonable framework. Stratigraphical evidence of the antiquity of pre-pottery Jericho had already been given by the links which could be established by the succeeding pottery phases with the Neolithic of the Yarmuk and Byblos.

But the new evidence carries back the pre-pottery settlement millennia before the first incipient settlements elsewhere, and poses entirely new problems. In the first place, it is clear that Jericho does not stand in isolation. The problem of against whom the defences were necessary to protect the site had always been puzzling. The only evidence of a contemporary population had hitherto come from the finds of Tahunian-type flints, which were the standard type of the plastered-floor levels. The finds of these flints elsewhere had come from surface sites or caves, nowhere suggesting an urban or even village population, and it seemed improbable that such large-scale defences were required to protect the compact and numerous Jericho population from nomadic and unorganised groups. But now there is a strong presumption that the defensive system represented by the tower and its associated walls and ditch were constructed by the hog-backed brick people as a defence against the plastered-floor people, who eventually succeeded them on the site.

Moreover, the plastered-floor people arrived on the site with a fully developed architecture, indeed an architecture which, from consistency of plan and structural methods, had been stereotyped by a long period of previous development. Therefore, the plastered-floor people came from urban sites. Presumably, these sites were not far distant from Jericho, for the earlier inhabitants would not have felt the necessity of fortifying themselves

unless their potential enemies were in the reasonably near vicinity. The most likely area is in fact the Jordan Valley, where there are many unexplored ancient sites, for environmental studies, and also our knowledge of the periods of regional development, suggest that the hill-country would have been too heavily wooded to be a likely area of early occupation and agriculture. It is possible, therefore, that we have to envisage, in the Jordan Valley at least, a fairly widespread growth of permanent settlement at the exceedingly remote period to which the evidence from Jericho points.

The second major problem posed is the relation of this early occupation, factually established at Jericho, and inferred elsewhere, to the hitherto known beginnings of village life in the Near East, for which a fifth millennium date is usually accepted. Here we have the evidence that the comparable stage at Jericho is one of retrogression. Between the last stages of the pre-pottery plastered floor occupation and the first levels in which pottery appears, there is a sharp stratigraphic and cultural break, though the evidence is so far ambiguous as to whether there was an actual interval. The people who introduced the first pottery, characterised by the fine, red-burnished decoration, entirely lacked the urban civilisation of their predecessors. The interpretation of the evidence which at the moment seems most probable is that they in fact built no permanent structures at all, but merely camped on the site. They were succeeded by another group, who used pottery characteristically decorated with bands of incised herringbone patterns. This group built houses, using bun-shaped mud-bricks, but the houses were still infinitely inferior to that of the pre-pottery people, and there is as yet no evidence of organised community life. It is this group that can be linked, by resemblances in pottery to the Sha'ar ha Golan site on the Yarmuk and to Byblos, to the period of incipient villages elsewhere in the Near East.

The fact that at Jericho a much more advanced civilisation preceded by some thousands of years the beginnings of village life as so far known requires a new hypothesis, a solution which must remain hypothetical until it is supported by evidence from elsewhere. If reliance can be placed on the Carbon-14 dates, which are at least supported by stratigraphical evidence of a very long succession, the beginnings at Jericho must approach the eighth millennium. We are thus carried back to a period not long after the end of the Pleistocene, for the usual dating given to the end of the Ice Age and the accompanying Pluvials in the Mediterranean area is c. 10,000 B.C. The hypothesis here put forward is that following the end of the Pluvials there was a period of favourable climatic conditions, with abundant vegetation, which encouraged a rapid growth of population and the evolution of at least sufficient elements of agriculture and stock-breeding to allow of the development of considerable permanent settlement. As desiccation gradually spread with the approach to modern climatic conditions, this premature flowering of civilisation was checked and eventually withered, to be succeeded in due course by new tentative beginnings which only reached a mature

civilisation again when the special conditions of the great river valleys provided the necessary stimulus.

It may be possible to test some aspects of this hypothesis when all the material from Jericho has been fully examined, for instance when all the animal bones and samples of grain and grain impressions have been examined, and when many more specimens have been submitted to a Carbon-14 test. But the full examination of the problem must await much more widespread investigation of other early sites, of the early levels which may lie in tells in the Jordan Valley and, further afield, in the heart of the great mounds of the Syrian plains.

Early Bronze Age

In three sites, Trench II at the north end of the tell, Trench III at the south end, and Squares E III–IV, excavations have been concerned mainly with the Early Bronze Age. In the northern and southern trenches, work was continued on the elucidation of the defences. Both areas showed a similar complicated sequence to that revealed in Trench I (*P.E.Q.* 1953). It is not possible at this stage to correlate the successive rebuildings on the different sites. In neither case has the original wall been certainly reached, and it by no means necessarily follows that any one rebuilding can be traced round the whole circuit. A broad correlation will probably be possible when the pottery has been worked over in detail, but the field inspection has not established any sufficiently salient points to relate the phases on the three sites.

In Squares E III–IV, the excavation of Early Bronze Age houses was continued, and the base of the levels of this period has very nearly been reached. Again, detailed work on the pottery has not yet been carried out, but there is a strong suggestion both from the pottery and the house plans that there are two main phases within the Early Bronze Age, each with a number of building stages. In the earlier, the buildings appear to be on a larger scale, and to be separated from each other by thick midden fills. In the later, there is a large number of rebuilds with only slight change in level, and the most noticeable characteristic is the number of brick-lined silos associated with the houses.

Intermediate Early Bronze–Middle Bronze Period

On the Tell, no levels of this period were excavated this year. But interesting new evidence was forthcoming from the tombs. It will be remembered that in the first three years' excavations, the tombs of this period fell into two sharply defined categories (*P.E.Q.* 1953), small neat tombs with single intact burials, with as grave-goods either a dagger or else a pin and beads, and very large tombs with single burials of jumbled bones and with a number of pots, almost invariably of tooth-brush-mug type, as offerings. These were interpreted as evidence of separate tribal groups, each with its own individual burial customs. In 1955, two new varieties were found.

In one, the tomb had a square shaft, the burial was intact, and both daggers and pots were found. In the other, the skeletons were jumbled and the offerings very poor, usually only beads and bronze studs. The square-shaft tombs resembled one of the types found at Tell Ajjul, and the differences may suggest further tribal groups, for the cemetery areas in which the tombs were found were mainly distinct.

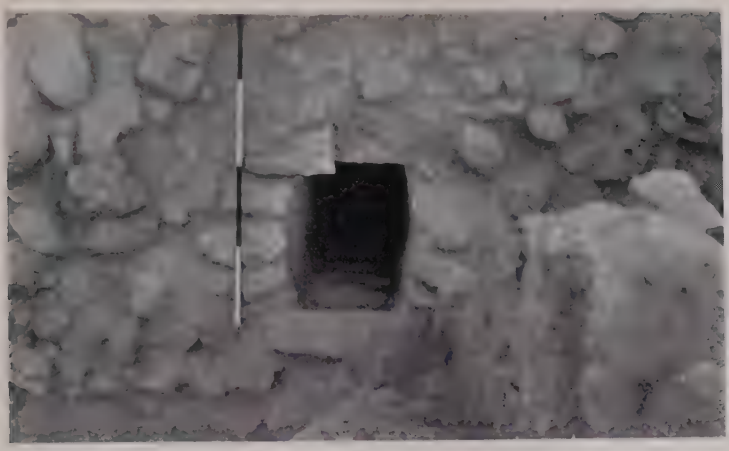
In 1956, a further variety was found. The makers of these seem to have been suffering from megalomania. The burials are again almost invariably single, but everything else about them is on an excessively large scale. The shafts are enormously wide and enormously deep, the largest being 4 metres wide and 5.88 metres deep, and the chambers ridiculously out of proportion to the single burials found in them. One, so large that it was christened " the Albert Hall," was 3.50 metres in diameter and 2.48 metres high, and contained a single burial of an individual who had obviously suffered badly from osteo-arthritis of the spine (pl. XIV. 1). The pots themselves differed from those of the familiar tooth-brush-mug type, and were swollen and bloated, and often very numerous. One tomb, again with a single burial, contained no less than twenty-two (pl. XV. 1). In some of these tombs, there were also daggers, and in two cases a javelin pl. XIV. 2). The burials were as a rule intact, and they thus combined features of the two main types, the Dagger Type and the Pottery Type. It is not as yet possible to say whether this combination of features represents a later stage, in which the tribes settled at Jericho were merging, or yet another tribal group.

The most interesting find of this period was in the very last tomb excavated. The shaft was on the same grandiose scale as the others found this year, 5.88 metres deep and 4 metres wide, but the roof of the chamber had had a serious roof-fall. One side of the shaft was formed by a natural rock cleavage, giving a beautifully flat vertical plane. On this it was discovered that a number of graffiti had been scratched (figs. 5 and 6). The majority are of horned animals, probably goats, some apparently browsing on trees, a curious anticipation of a familiar motive of the Late Bronze Age. In addition there are drawings of two little men, holding in their left hands square shields and in their right, long shafts. There is little doubt that these shafts represent the javelins, of which three examples of the heads have now been found in these tombs. These javelin heads with curled ends to the tangs have been found in tombs of this period at Tell Ajjul and Tell Duweir. In the Jericho tombs, it had been observed that the characteristic position of the javelin-head was well above the right shoulder of the skeleton (pl. XIV. 2), which would confirm that they had been attached to a long shaft, as the graffiti suggest.

The drawings are the first evidence of any sort of artistic impulses in the nomads who destroyed the Early Bronze civilisation of Jericho and much of Palestine. They are certainly not great art, but their interest is nevertheless considerable.

The defences of pre-pottery Neolithic Jericho; ditch, wall and tower behind.

The great tower of the pre-pottery Neolithic defences of Jericho; the outer skin-wall has been removed.



1. The entrance to the stairway in the tower.

2. The steps leading down into the heart of the tower.

1. The back of the great town wall, built against the outer skin-wall of the tower.

2. On left, the great wall contemporary with the third stage of the tower, in centre, the wall probably contemporary with the first stage.

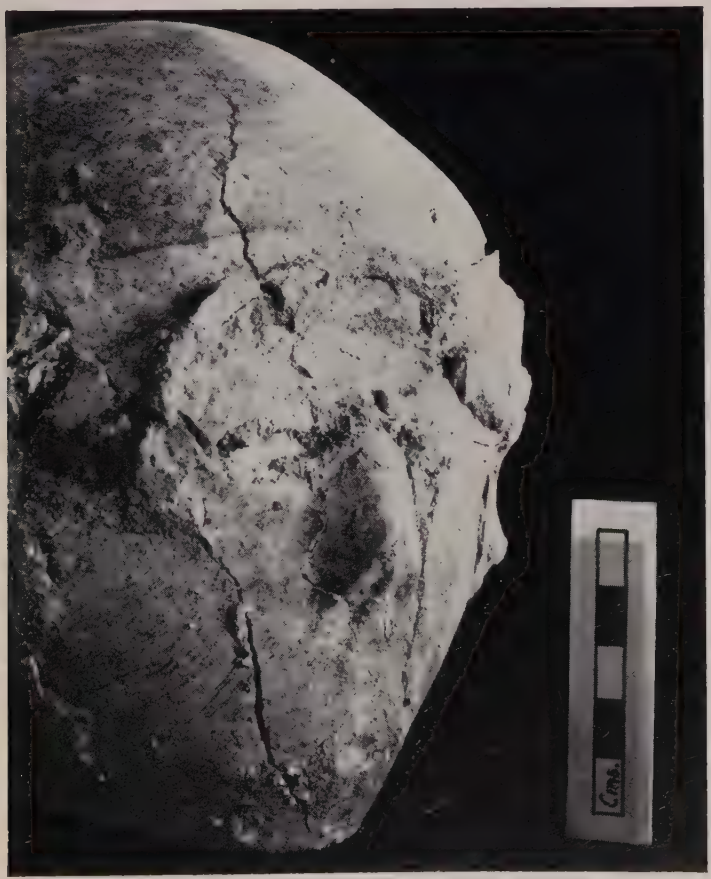
1. Part of a building of the type characteristic of the earlier pre-pottery Neolithic culture.

2. Steps which originally had wooden treads, leading down into a house of the earlier pre-pottery Neolithic culture.

1. The stream-bed which cut through the buildings of the earlier pre-pottery Neolithic culture; the first plastered floor is visible just above the upper black section of the ranging pole.

2. Above, a brick of the hog-backed type; below, a brick of the flattened-cigar-shape-with thumb-impressions

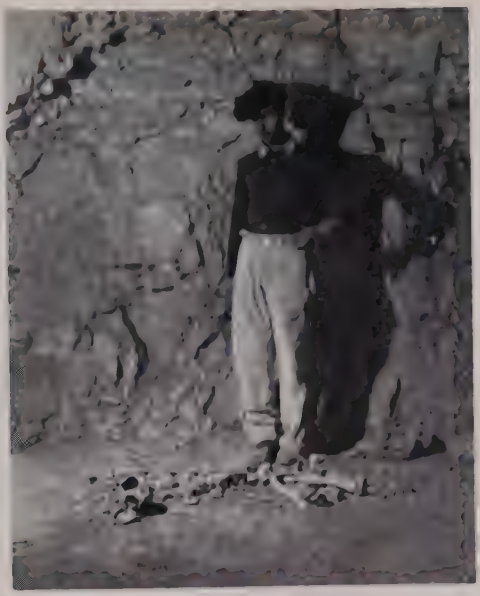
1. Two human skulls with plastered features.



2. Plastered skull with, on left, traces of a moustache in paint.

Group of skulls in Square E II, of earlier pre-pottery Neolithic culture.

Group of skulls in Square D I. of earlier pre-pottery Neolithic culture.



1. Single burial in very large tomb, of Intermediate Early Bronze—Middle Bronze period.

2. Javelin and pots in tomb of Intermediate Early Bronze—Middle Bronze period.

1. Tomb of Intermediate Early Bronze – Middle Bronze period with single burial and twenty-two pots.

2. Brick-built tomb of the Middle Bronze Age on the Tell.

Typical Middle Bronze Age tomb, with bones and offerings of earlier burials pushed to the rear.

Fig. 5.

Middle Bronze Age

The excavation of the great defensive bank with its plastered surface had in earlier seasons been completed in Trenches I and III, and this year was completed in Trench II. The only feature in which it here differed from the other sites was that the flat berm between the foot of the plastered scarp and the outer retaining wall was appreciably wider. Here, as elsewhere, the retaining wall belonging to the earlier phases had been removed when the later walls were built, so that the great wall of the third phase, which has been traced round most of the circuit of the Tell, is the only one of this feature of the defences to survive.

The cumulative evidence of the three trenches, together with that of the Middle Bronze Age buildings on the east side of the Tell, now makes it possible to make a general observation on the lay-out of the Middle Bronze Age town. The only point at which the city wall which crowned the plastered scarp survives is at the north-west corner of the Tell, where it is at a height

of 21 metres above datum. This is the highest surviving point of the Tell. The area immediately to the east has been much cut about in previous excavations, so its original contours do not survive. But apparently the latest buildings were of the Early Bronze Age, at a height of about 17 metres. The Middle Bronze buildings have therefore been removed by denudation. But it seems extremely improbable that more than 4 metres of deposit in the centre of the Tell has been washed away at a place where the outer edge is

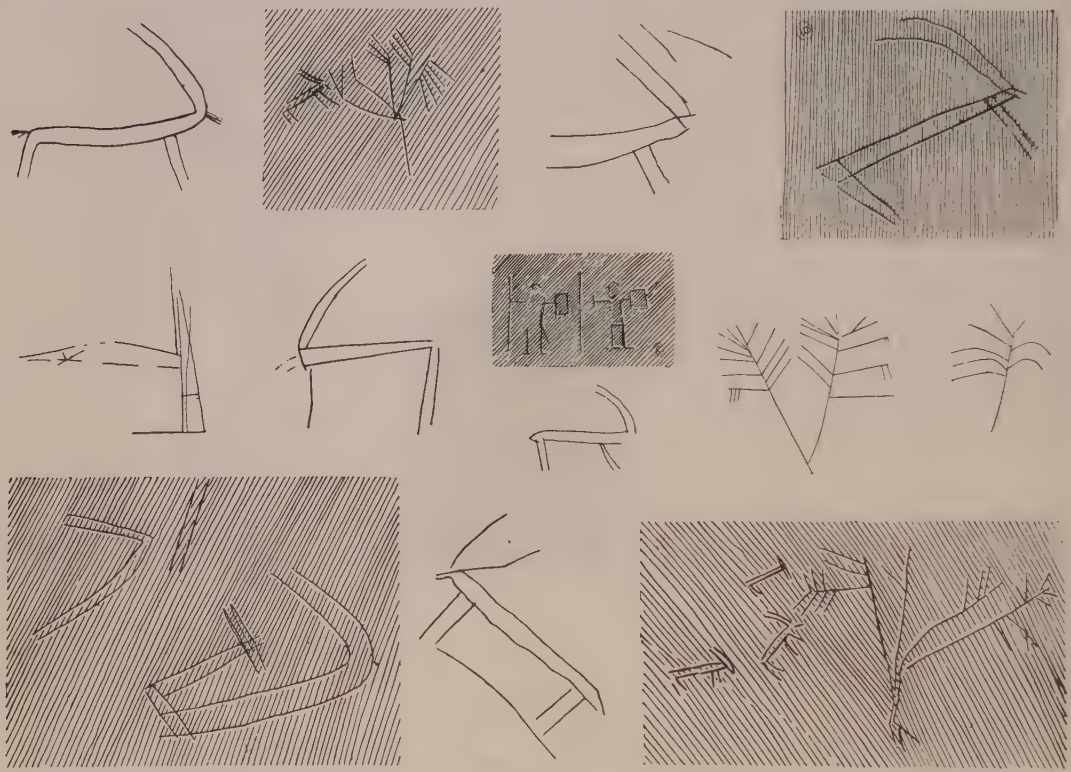


Fig. 6.

intact. Therefore, the Middle Bronze Age rampart-bank probably also sloped down on the inner side, thus resembling in profile the defensive banks of a British Iron Age hill-fort, or, more appositely, the contemporary plaster-faced banks of Tell el Yahudiyeh in Egypt or Qatna in Syria, the difference being that in these sites the banks stand up from a plain, whereas at Jericho, on the western side of the site, they crown an already existing mound. But it can be further deduced that on the east side they probably did *not* crown the mound, and were built, like the other two examples, on the flat ground. There are two pointers to this. The Austro-German expedition traced the great revetment forming the foot of the final stage for some distance out into

the cultivated ground at the base of the Tell on the east side, suggesting that here they swing away from the old mound, presumably more effectively to enclose the spring. It is clear that the lowness of the Tell on this side is not due to erosion, for it is on the east side alone that Middle Bronze Age houses survive. The floor level of these houses at the eastern edge (they are built in terraces running up the hill, see *P.E.Q.* 1954) is at 8 metres above datum. It is clear therefore that the defences must have consisted of a free-standing bank, beyond the foot of the earlier city mound.

The excavation of the Middle Bronze Age houses on the east side of the Tell has now nearly been completed. It has been complicated by a series of terraces, some of which had partially collapsed, and by the existence of some deep vats or cisterns lined with mud plaster. Some eight main building phases below the building probably destroyed by the Egyptians c. 1580 B.C. have been identified. The earliest pottery dates back to comparatively early in the Middle Bronze Age, but has not yet been fully examined. The most interesting find was that, belonging probably to the earliest stage, there was a brick-built tomb (pl. XV. 2). It was a rectangular structure, 1.75 metres by 1.25 metres, with an entrance at one short end. The walls only survived to a height of .50 metre, but from the way that they were inclined in it appeared that the roof was corbelled. The burial customs were similar to those of the rock-cut tombs, with successive multiple burials, the earlier remains being pushed to the rear to make room for the later burials.

A number of Middle Bronze Age tombs have been excavated in the cemetery area, again with most interesting remains of wooden objects, basketry and textiles. A view of a typical tomb, with the bones and offerings of the earlier burials pushed to the rear, is given on pl. XVI. In one tomb, there had been, as in those excavated in 1954, mass simultaneous burials with a row of 9 bodies laid out side by side. The most interesting tomb, however, was first used earlier than any so far found. It very closely adjoined another tomb, of the Intermediate Early Bronze–Middle Bronze period, and between its first Middle Bronze Age use and the final one, the wall between the two tombs collapsed. Therefore, when it was reopened for its final use, the adjoining tomb was used as a repository for many of the objects belonging to the earlier burial. These are typical of Middle Bronze I, and can be equated with Tell Beit Mirsim strata G–F. Apart from the burial on the Tell referred to above and from an infant burial in the same area (found in 1955), this is the first tomb of this period that has been found. Further excavation might show that burial on the Tell was the normal practice at this period.

Roman Period

Several more tombs, or rather earlier tombs re-used, of this period were found. The most interesting had burials in wooden coffins with gable-shaped ends, the wood being remarkably well preserved. In one coffin, the head of the skeleton rested on a leather pillow.

Byzantine–Ommayad Period

On the east side of the Tell, two new squares were begun in the hope that more would be found of the Late Bronze Age house found in 1954 (*P.E.Q.* 1954). However, after the shifting of a considerable dump, it was found that in Square H V previous excavations had penetrated below this level. In Square H IV, however, a substantially built little house was found immediately beneath the surface. In it were many objects typical of the Late Byzantine–Early Ommayad period, probably contemporary with the Ommayad Palace at Khirbet Mefjir near by. This area is therefore untouched by previous excavations, so there is hope that some Late Bronze Age levels may here survive.

AN APPROACH TO THE PROBLEM OF OLD TESTAMENT MYTHOLOGY

G. Henton Davies

The problem of an Old Testament Mythology is generally and rightly considered to be a difficult one. The question is difficult because mythological material from the ancient Near East dwarfs any comparable material in the O.T., and so tends at first sight to depreciate the significance of that O.T. material. It is also difficult because of the natural disinclination to think of any important part of the O.T. material in terms of a mythology. Indeed to find a mythological theme dominant in the O.T. as similar themes are dominant elsewhere has scarcely seemed possible.

The questions that thus arise are: Is there a mythology in the O.T. ? If there is, is that mythology simply one that has been taken over by the storytellers and poets of Israel and in their adoption transformed by the influences of Israel's religion ? Lastly, can any claim be made that Israel developed its own mythology which might be discovered in the O.T. alongside any borrowed or transformed elements ?

It is also fortunately true to say that there are the beginnings of a history of investigation into the subject, and various books have recently been published to set us on our way, and it is one of these that may serve as our starting point.

Christian Hartlich und Walter Sachs in their book *Der Ursprung des Mythosbegriffes in der Modernen Bibelwissenschaft*, have traced the history of the investigation over the last two hundred years. It was Bishop Lowth in his work *De sacra poesi Hebraeorum* 1753 who brought a new spirit into the study of the O.T., and with his idea of *stilus parabolicus* laid the foundation both for the study of Hebrew poetry, and for the comparison of Biblical and ancient non-biblical material. He thus made possible the " introduction of the conception of myth "[1] into Biblical study."

Following Lowth, C. G. Heyne studied mythology from the psychological, historical and philosophical standpoints, and emphasising the antiquity and universality of mythology, classified myths as (*a*) historical, and (*b*) philosophical (philosopheme). Then J. G. Eichhorn and J. P. Gabler began to apply Heyne's methods and findings to the Biblical material, and thus became aware of the many problems arising from the mythological study of the O.T. and of the N.T.

[1] Hartlich-Sachs. op. cit. p. 10.

With G. L. Bauer (1755-1806) there emerges the scientific study of mythology. Bauer investigated the difference between true history and myth. The following quotation from Bauer's work appears in Hartlich and Sachs, and illustrates Bauer's findings:

> " The distinguishing marks by which a myth may be recognised and distinguished from a true story are as follows: First: when the origin of the world and of the earth, of which no one was witness, is recorded: Secondly: when in place of natural causes, through which things are brought about, everything is traced to the activity of the gods or heavenly beings, who personally appear and directly take part: Thirdly: when everything is physically presented, and men speak and behave where they have only meditated: Fourthly: where the narrated affair is so conceived that it never does happen or according to the ordinary course of nature could not have happened, but passes all belief ".[1]

As Hartlich and Sachs[2] point out G. L. Bauer's *Hebräische Mythologie des Alten und Neuen Testaments* has remained the principal investigation into Biblical mythology down to the present day.

W. M. L. de Wette (1780-1849) described myth as the category normal for the expression of the religious life and spirit. So fundamental and regulative is the mythological standpoint that it is idle to speak of different classes of myths as hitherto, though he did recognise that there was a naive, a dogmatic and a critical use of mythology. This recognition of the importance of the mythological norm for the interpretation of the O.T. material will of course remain fundamental for Biblical study.

The Hartlich-Sachs investigation has shown

(i) that there existed various types of myths in the Bible.

(ii) that much of the Biblical material was permeated with a mythical spirit and mode of expression that often made the differentiation of mythical and historical features impossible and undesirable.

(iii) that Biblical myths could not claim that they alone corresponded to reality, for all mythical material, so far as it is unverifiable, is the same in or outside the Bible. Biblical myths then do not possess in their own right any special position. It is accordingly not possible to countenance any idea of accommodation on the part of the Biblical writers. P. Heinisch[3] quotes, apparently with approval, the words of St. Gregory of Nyssa, who said: " Sacred Scripture often indulges in culling from matter that is definitely mythological to attain its own purposes: . . . Did the sacred writers believe these myths ? By no means ".[4] The view of accommodation implicit in these words stands condemned in the light of modern biblical study.

[1] Hartlich-Sachs. op. cit. p. 77.

[2] op. cit. pp. 79 f.

[3] *Theology of the Old Testament*, p. 150.

[4] In Cant. Cant. Hom 9. M.G. 44. 973.

It is unnecessary to do more than merely mention the Pan-Babylonian School or Pan-Orientalism, for its findings, at least in the extreme form, remain a curiosity of scholarship.[1] At the base of their work there was of course a fundamentally sound conception, and this has emerged in truer and more lasting form in the British School of Myth and Ritual, and in the general idea of Patternism which has resulted therefrom. Let me note at once that Israel's material and spiritual culture must be understood against the background of the Near Eastern environment. There seems to me to be no doubt that subject to three qualifications it is Israel that has been the borrower. These three qualifications are

(*a*) Israel has undoubtedly borrowed from the Near Eastern environment, but allowance must be made for institutions and ideas in Israel which though not borrowed are in fact comparable because parallel. Stories of theophanies and the institution of covenants will illustrate the point.

(*b*) The undoubted borrowing in no way precludes, as Professor I. Engnell, for example, says, " a remodelling activity according to ideas and ideals which stamp the whole with a very distinctive mark ".[2] Incidentally Professor Engnell does not say what these ideas and ideals were, or where they came from. This remodelling activity has now become one of the clichés of O.T. study in regard to the mythological material, and it is echoed by almost all writers on the subject. A notable statement of this remodelling activity is to be found in a short article by H. G. May,[3] who describes how each successive generation interprets its experience in the light of its mythological patterns which are inherited from the past or borrowed from neighbouring nations.

(*c*) The third qualification is really concerned with a possibility arising from these same ideas and ideals, which give a distinctive mark to Israel's culture. It must be asked whether such distinctive ideas and ideals were really indigenous in Israel. What is it that gives a distinctive mark to Israel's religion ? Further are these distinctive ideas and ideals in fact so distinctive as to be worthy of the claim of originality ? Granted then the fact of Israel's borrowing, and granted too the distinctive process of transforming or remodelling in the Old Testament, is it possible also to claim original elements in the Old Testament ? Israel did borrow myths and preserve them, transforming them in the process. The question is, and this is our problem, did Israel create any myths ? Does the Old

[1] See e.g the criticism in W L. Wardle, *Israel and Babylon*, 1925, c. 12.

[2] In his review of J. de Fraine's work " L'Aspect religieux de la royauté israélite ", 1950, in *Svensk Exegetisk Arsbok*, Vol. XVIII-XIX. 1953-54. p. 203.

[3] " Pattern and Myth in the Old Testament ", in *The Journal of Religion*, Vol. XXI. July 1941, pp. 285-299. cf. also E. Bevan. " The Religious value of Myths in the Old Testament " being c. XXXI in *The Clarendon Bible O.T.* Vol. VI, *In the Beginning* by S. H. Hooke, 1947, pp. 149-162.

Testament contain any signs of a mythology which is Israel's own creation ?

There have not been wanting those who deny the presence of a mythology in the O.T.

Hartlich and Sachs[1] point to the work of J. J. Hess in 1792 as the first reaction against the idea of the mythology of the O.T. He claims that mythology belongs to polytheism, and that a monotheistic Bible cannot contain any myths. It is substantially this point of view which is echoed as we shall see by A. Weiser in 1949. Weiser claimed that the cradle of mythology is polytheism, and that the presuppositions for the making of myths are lacking in the O.T. The monotheistic and historical tendencies of the O.T. hinder myth-making in the O.T. and lead to the modification and to the destruction of borrowed mythological material.

On the other hand attempts have been made to describe mythology in the O.T.

Thus Gunkel[2] describes the nature, astrological, and eschatological myths of the O.T., and traces myths which have been borrowed or transformed. He also speaks of reminiscences and remnants of myth in the prophets and poets of the O.T. and in the abstract ideas of the O.T. Gunkel is inclined to think that in the theophanies of the O.T. at Sinai, in Deut. 33. 2, Jdg. 5.4, and in the Elijah stories, the material may be of Israelite origin, even if such stories only served as a prelude to the struggle between the gods. But the myths are mainly borrowed and transformed.

Similarly T. H. Robinson[3] has reviewed Hebrew myths, and, considering material comparable with that presented by Gunkel, finds three classes of myths in the O.T. : (*a*) cosmic and Nature myths belonging to the pattern; (*b*) myth fragments loosely or not all connected with the pattern, such as the flood and sun myths, and (*c*) a moon myth belonging to the " cult of some other deity " and now transferred to Yahweh in connection with the Passover. The Israelites brought with them into Palestine a myth and ritual which they connected with the Passover as their most ancient and characteristic festival.

Quite recently J. Hempel has published a review of Israel's mythology consisting of three articles from *Z.A.W.*[4] He speaks of three circles of myths: (*a*) cosmogonic myth and its sequel in eschatology; (*b*) soteriological myth and its connection with saviour types in the O.T.; and (*c*) revelation myth as this is seen in the O.T. theophanies and other revelatory media. In the material relating to the beginning and end of the world, there can of course be no question of a gulf between myth and history, but a demythologising process can be traced in the O.T. In fact the monotheistic character of

[1] op. cit. pp. 58-61.

[2] *R.G.G.* Band 4. Art. " Mythus und Mythologie III A, im A.T.".

[3] " Hebrew Myths ", being c. VIII in *Myth and Ritual*, ed. by S. H. Hooke, pp. 172-196.

[4] *Glaube, Mythos und Geschichte*, Sonderdruck aus *Z.A.W.* Band 65.

Yahwism which has controlled the canon, has obliterated or otherwise adapted the myths of divine lamentation and marriage.[1]

Similarly Millar Burrows writes of the mythological world-view of the Bible and mentions philosophical, that is, cosmogonic, ritual and astrological myths.[2]

These sample reviews of myths in the O.T. show beyond doubt that Israel used and enjoyed myths. Israel was φιλομῦθος, and the O.T. indeed shows some evidence of the mythopoeic way of thinking.[3] Nevertheless these same writers believe that the myths of the O.T. are borrowed myths. They believe and rightly that it was inevitable that Israel should borrow myths from the cultural background of which she was a living part. Israelite poets then modified, adapted, remoulded these myths so that they employed the myths as a vehicle for the ethical and monotheistic faith so distinctive of the O.T. Indeed this Israelite remodelling took the form of history, the poetic fiction of the borrowed myths was adapted to the historical. The dragon became Egypt and so on.

It is however also clear that these same writers are reluctant to suggest that Israel could create a mythology. There is Gunkel's suggestion that Israel may have created some of the theophanic myths but this touch of originality was soon lost in patternism, because the myths of the theophanies were used to introduce the myths of the divine combats.

It is sad to think that a people which borrowed myth, and used myth, and transformed myth, could not yet create its own myths. There must be a reason for this inability. Thus Gunkel writes: " Israel was never favourable to myths. The myth is by its nature polytheistic, and very often presents the deity in close connection with nature ".[4]

Eissfeldt carries on the same thought. He agrees that the Israelite spirit is highly gifted for mythological conceptions, but was never able to exercise her gifts. The portrayal of such theophanies as Ex. 19. 33. 19-23, Judg. 5, 4-5, I Ki. 19. 9-18, and Ps. 18. 1-6, shows that Israel was capable of creating a mythology. " But ", he says, " proper myths do not occur in the O.T., at least none that arose in Israel, rather only those borrowed from outside. That is doubtless bound up with the henotheistic nature of Israel's belief in God, which recognised as the proper, effective and authoritative God, only the one Yahweh, Israel's tribal and national God. Real myth assumes at least two gods, a principal character and his opposite number . . . what

[1] Hempel. op. cit. p. 5.

[2] *An Outline of Biblical Theology*, 1946, pp. 113-116.

[3] See H. Frankfort, et alii. " Myth and Reality " being c. 1 of *The Intellectual Adventure of Ancient Man*, pp. 3-27. Examples of mythopoeic thinking in the O.T. may be seen for example in the repeated quest for the ark in Ps. 132. 6-8, and in such words as " Yahweh made not this covenant with our fathers, but with us, *even* us, who *are* all of us here alive this day ", (Deut. 5. 3. etc.).

[4] Op. cit. p. 382.

lies before us in the O.T., in the shape of myths or what is in accord with myths, has quite clearly penetrated into Israel from outside . . . ".[1]

A. Weiser shares the same point of view when he claims: " The presuppositions for myth-making are lacking in O.T. religion. Its monotheistic tendency and its organic connection with history, standing as it does in a certain tension to nature religion, tended indeed to hinder real myth-making, as it also subjected the adoption of foreign myths to a radical transformation, so that in the O.T. only remnants of decaying myths or mythological motifs remain ".[2]

Bentzen too says that myth " generally presupposes *polytheism* and accordingly has not had favourable conditions in Israel. The religious history of Israel is in some respects a history of ' de-mythologization ' ".[3]

It is accordingly not surprising to read Stählin's bald statement: " Myth is a heathenish category. Even if it still appears in many parts of the O.T. in ' Rudimenten ' and in the N.T. in ' Metamorphosen ' . . . myth as such has no place in the Biblical ground whether as first-hand communication of religious ' truths ', or as Parable or as Symbol ".[4]

The quest for a mythology indigenous in the O.T. seems a forlorn if not impossible task. Even if Israel could have created its own mythology, conditions were unfavourable for its development.

There seem to me to be not less than three errors in the description of O.T. myths so far reviewed.

(*a*) There is first the fundamental objection that these writers do not recognise that mythology is a way of thinking and of imagining about the divine rather than a thinking or imagining about a number of gods. Myth is a record of the numinous, whether the numinous is single or manifold. Frankfort thus summarises his description of myth: " Myth is a form of poetry which transcends poetry in that it proclaims a truth; a form of reasoning which transcends reasoning in that it wants to bring about the truth it proclaims . . . "[5]. Strictly speaking then, the content of the myth, whether polytheistic or otherwise is accidental to the nature of myth. Myth is a way of thinking, independently of a polytheistic setting.

(*b*) Secondly, it must be questioned whether the application of such terms as polytheistic, henotheistic to the divine in the O.T., or in the ancient Near East, is justified. In his 1950 Gifford lectures, published in 1954, one of our most distinguished English divines, H. H. Farmer, says: " What we have to realise is that though notionally or theologically, polytheism and monotheism are exclusive of one another, they are by

[1] O. Eissfeldt. *Einleitung in das Alte Testament.* 1934, p. 36.

[2] A. Weiser. *Einleitung in das Alte Testament,* 1949, p. 50.

[3] A. Bentzen. *Introduction to the Old Testament* (2) 1952. Vol. 1. p. 241.

[4] *T.W.N.T.* Band IV. p. 800.

[5] H. Frankfort. op. cit. p. 8.

no means wholly so when seen from within the religious experience itself".[1] Farmer also describes how a monotheistic faith could break down by failing to keep separate from one another the symbol and that which it symbolises, and how to multiply the symbols of a god could mean multiplying the one deity, especially in the absence of a full faith in one God.[2]

Drawing, as Farmer has done, a parallel between psychological and theological thinking, A. R. Johnson has shown the richness of Yahweh's personality in terms of the one and the many.[3] Everybody is familiar with the variety within the personality of Yahweh. There is His Spirit, His Word, His Name, His Glory, His Face, His Angel; Sons of God, a Son of God, or the Gods of Yahweh's court; His Ark, His Tent, His Temple; all these are listed and may be studied in almost any theology of the O.T. The merit of Johnson's work is that by his conception of the extension of personality he is able to think of Yahweh in terms of a collective or corporate personality.

Here then in the conceptions of the extensions of personality, and of corporate personality within the personality of Yahweh, may be found the material for a mythology in the O.T. Of course these conceptions are not unique to Yahweh and Israel. They are simply the Israelite equivalent of similar ideas found in the Ancient Near East. But at least the objection that there could be no mythological themes in the O.T. because of the anti-polytheistic interest, is of course an unsound interpretation.

In the same way H. H. Farmer has drawn attention to unifying tendencies in polytheism. He points out that " self-conscious experience is not possible without some sense of the unity of self and of the unity of the world apprehended by the self ".[4] Therefore it follows that in some sense " all the gods, despite their separateness, belonged together in the unity of one ultimate supernatural reality ".

Farmer also points to the element of concentration in religious experience.[5] This is obvious in prayer, but it cannot be denied that in the so-called polytheistic myths, attention seems to be concentrated on one deity, so that myths tend to revolve around one leading deity, though other deities are of course mentioned.

(*c*) The third error is that those writers who have written recently on mythology in the O.T., are apparently of the opinion that the only mythology available is the mythology of patternism, or of the " seasonal pattern " as Gaster describes it.[6] This error follows inevitably from their pre-conception about the polytheistic character of mythological thinking. Once we can be convinced that mythology is a way of thinking, then it

[1] *Revelation and Religion*, p. 100.

[2] op. cit. p. 101.

[3] *The One and the Many in the Israelite Conception of God*, 1942.

[4] op. cit. p. 104.

[5] op. cit. p. 105.

[6] T. H. Gaster, *Thespis*, passim.

will be seen that polytheistic and patternistic types are not the only types of mythological thinking. Both the number of the gods involved, and the kind of story that is told about them are not primary facts in mythology. It must be emphasised that mythology is first and essentially a way of thinking or imagining about the divine.

It is therefore possible to approach the problem of mythology in the O.T. apart from the facts of borrowed and transformed mythology. It is possible to think of a mythology indigenous in the O.T. in terms of the personality and activity of Yahweh, the God of Israel.

We have seen above that Gunkel was prepared to find genuine Israelite mythological material in the stories of the theophanies. Eissfeldt too paid tribute to Israel's mythological capacity as this was presented in these same stories. They both cite the Sinai stories, Jdg, 5. 4-5, the Elijah story in I Ki. 19 and Ps. 18. In turn we note that these theophanic stories, etc., are controlled by the idea of the " Presence " of Yahweh. These theophanic stories and descriptions are the vehicle of the " Presence " of God in varying forms and manifestations. An English writer, W. J. Phythian Adams, has devoted several books to this idea of the Presence of Yahweh in Israel. In his most notable work from this point of view *The People and the Presence*, he has established the idea of the Presence as one of the dominant themes of O.T. investigation. It should of course be realised that this author has written from within a particular christian tradition, that he has drawn certain conclusions about the difference between the idea of the Presence in the O.T. and in the N.T. which are not really justified, especially in regard to the O.T., and that he has introduced various speculative considerations which do not help his theme. But when allowance for these things has been made, it is nevertheless true to say that he has revived an aspect of O.T. religion and faith which has long been neglected.

The idea of the " Presence ", the " Tabernacling Presence ", is of course, a mythological idea, and it is an idea that may be traced throughout the whole of the O.T. Granted that the idea of theophany is by no means peculiar to Israel, and that theophany is a universal idea within and without the Ancient Near East, nevertheless the record of the " Tabernacling Presence " both as to its origin, its history and its recurring pattern appears to me to be genuinely Israelite. It is the plot of the " Presence ", the recurring drama and so the total idea of the " Presence " which confers uniqueness upon the mythology of the O.T. The problem of O.T. mythology can find its solution in the presentation of the theme of the " Presence " which the pages of the O.T. unfold to us, and of the theme of " Realised Eschatology " as its sequel in the N.T.

In his list of " images[1] which are the vehicles of O.T. revelation ", N. W.

[1] Whether the terms " images " or " symbols " are used is really immaterial, for in this context they are the products of thinking about the gods or God.

Porteous has included " the Presence in the Temple ".[1] This shows that the idea is beginning to gain some recognition. Many of the older studies in O.T. theology did not mention the idea of the " Presence ".[2] It was simply ignored, and it is this ignoring of the idea, continuing still in many quarters, which is difficult to understand. Surely no objective and scientific account of the O.T. can review the contents of the O.T. and pass over an idea which figures prominently in its pages ? There is the account of the " Presence " of Yahweh in Exodus. There are also the ideas of the " Presence " in the " Shem " (שֵׁם) theology of Deuteronomy, and the Kabhodh (כָּבוֹד) theology of P., and of Ezekiel, of the pillar of cloud, of tent and of ark, of tabernacle and of temple ; then there is the association of the " Presence " with Moses, with the king, with the priest, with the prophet and so on. There is also the re-appearance of the " Presence " idea in the eschatological portions of the O.T.

On the one hand then there is the claim that Israel for various reasons never developed its own mythology. On the other hand there is this very considerable body of material which deals with the " Presence " and which has been overlooked in scholarly writing. It is my concern to put these two ideas together, and to find the most important branch of the O.T. mythology in the portrayal of the tabernacling of the " Presence " of Yahweh in the various parts of the O.T.

Of course Pedersen was already pointing in this direction with his description of the Passover Legend as a " cultic glorification ". C. R. North looked in the same direction when he described the narrative of the Book of Exodus as history transformed and transfigured by faith. The narrative has the character of " Heilsgeschichte ". In the same way S. Stein has recently coined the word " Heilsgesetz " to describe the laws of the O.T. It is but a very short step from Heilsgeschichte to Göttergeschichte or Gottesgeschichte, and that is in fact mythology.

We may attempt, then, to resolve the problem of mythology in the O.T. in three ways.

(*a*) There is first the recognition that Israel has borrowed and adapted rites and myths known to us in the Myth and Ritual Pattern of the Ancient Near East.

(*b*) There is secondly the recognition that Israel has borrowed and adapted myths and fragments of myth not essentially related to the pattern.

(*c*) There is the claim that like her neighbours Israel was gifted with the capacity to think and to enjoy mythology, and developed her own peculiar mythology within the orbit of Yahweh's personality, and in particular in reference to the Presence theme now discoverable in the pages of the O.T.

[1] " Semantics and Old Testament Theology " being c. 1 of *Oudtestamentische Studien Deel*, VIII. pp. 1-14. v.p. 10.

[2] As the tables of contents in O.T. theologies and works relating to religious ideas in the O.T. abundantly show.

THE KITTIM AND THE DEAD SEA SCROLLS

By H. H. Rowley

The area of legitimate disagreement about some major problems raised by the Dead Sea Scrolls is being steadily limited. When the Scrolls first came to light there was the widest possible divergence in the dates given for the composition of the texts, ranging from the early part of the second century B.C. down to the Middle Ages. The excavation of the centre occupied by the sect at Khirbet Qumran should by now have excluded many of the dates that were suggested. Prior to that excavation Father de Vaux had maintained that the deposit of the Scrolls in the cave—at that time only one cave was in question—could not have been later than about 100 B.C.[1] When, in consequence of what he found in the first excavation of Khirbet Qumran, he brought this date down to a little before A.D. 70,[2] I preferred to leave open the possibility that the deposit might have been as late as *circa* A.D. 135,[3] since the evidence did not seem to be conclusive for one of these dates as against the other. Since then a fuller account of the evidence has been made available, and further operations on the site have yielded new evidence.[4] This now seems to me to be conclusive for the earlier of these dates.

This still left open three possible dates for the Teacher of Righteousness. There was the possibility that he belonged to the early part of the second century B.C., and to the stormy events that included the Maccabæan rising. There was the possibility that he belonged to the time of Alexander Jannæus, when again deep inner divisions amongst the Jews are attested. And thirdly, there was the possibility that Dupont-Sommer was right in his argument for a date in the middle of the first century B.C., and for the death of the Teacher of Righteousness between 67 and 63 B.C.[5] No other possibility seems now to be allowed for by the archæological evidence.

While this third view has gained a good deal of support, it is likely that it will soon join those definitely excluded. It has always seemed to me to allow insufficient time for all that took place between the death of the Teacher of Righteousness and the deposit of the Scrolls in the caves. We must

[1] Cf. *R.B.*, lvi, 1949, pp. 234, 586 ff.

[2] In his paper read to the Académie des Inscriptions et Belles Lettres on April 6th, 1952, and published in *Comptes Rendus*, 1952, pp. 173 ff, and reported in *The Manchester Guardian*, April 7th, 1952, and *Le Monde*, April 9th, 1952.

[3] Cf. *B.J.R.L.*, xxxv, 1952-53, pp. 135f., and *The Zadokite Fragments and the Dead Sea Scrolls*, 1952, p. 88.

[4] Cf. R. de Vaux, *R.B.*, lxi, 1954, pp. 206 ff., *Comptes Rendus*, 1953, pp. 310 ff., 1955, pp. 378 ff., and G. L. Harding, *I.L.N.*, September 3rd, 1955, p. 379.

[5] Cf. *Aperçus préliminaires*, 1950, p. 47 (E. Tr. by E. M. Rowley, 1952, p. 35).

allow some interval of time between the death of the Teacher of Righteousness and the composition of the texts that reflect it, and some further interval between the composition of the texts and the copying of the surviving manuscripts, and then another interval between that copying and the deposit in the caves. A period of about one hundred and thirty years seems scarcely long enough for this, especially in view of the evidence for the long use of some of the Scrolls.

It is not this consideration which seems likely to make the view untenable, however. A far more serious difficulty is raised by the column of the Nahum Pesher, which J. M. Allegro has recently published in the *Journal of Biblical Literature*.[1] Here for the first time in any of these texts do we find mention of contemporary historical characters by their own proper names. There is mention of an Antiochus,[2] and also of a king of Greece, the termination of whose name alone survives.[3] This termination is *-tros*, and I think Allegro is right in holding that it must be Demetrius. This cannot be later than Demetrius III. The text further refers to one whom it calls " The Lion of Wrath ", who " hung men alive ",[4] and Allegro believes that this means that he crucified them. While this is not absolutely certain, I think it is probably correct. There is no mention of the Teacher of Righteousness, but there is a reference to " the rise of the rulers of the Kittim ".[5]

Allegro connects the events here indicated with the time of Alexander Jannæus, and recalls his crucifixion of eight hundred of his enemies.[6] This does not seem to me to be at all certain, as I shall say later; but in view of the references to Antiochus and Demetrius it is improbable that anything later than this is meant. Allegro rightly holds that the bitter hostility towards the person responsible for " hanging men alive " points to the probability that members of the sect suffered at his hand. He also assumes that the Teacher of Righteousness was crucified by him. This seems an odd deduction to make from a text which does not mention the Teacher of Righteousness.

It is possible that this persecution took place after the Teacher of Righteousness was dead, and this would offer a more reasonable explanation of the failure to mention him than the supposition that he was the principal victim of the massacre. It is highly improbable that this persecution took place before the time of the Teacher of Righteousness. For the text charges the priests of Jerusalem with gathering wealth,[7] and in the Habakkuk Pesher a similar charge is made against the Wicked Priest,[8] who was contemporary with the Teacher of Righteousness, and also against " the last priests of

[1] Cf. *J.B.L.*, lxxv, 1956, pp. 89 ff.

[2] Line 3: " from Antiochus to the rise of the rulers of the Kittim ".

[3] Line 2: " . . .] trôs, king of Yavan ".

[4] Lines 5 ff.

[5] Line 3; see note above.

[6] *J.B.L.*, loc. cit., p. 92.

[7] Line 11: " it is the wealth which the [prie]sts of Jerusalem gath[ered] together."

[8] Col. VIII, lines 11 f.

Jerusalem ",[1] while a small fragment of another Scroll cited by Allegro makes mention of the last priest and also of the " Lion of Wrath ", who would appear to be contemporary.[2] Allegro would identify the " Lion of Wrath " with the Wicked Priest.[3] While this does not seem to me to be certain, it is probable that at least he was contemporary with the Wicked Priest, and the latter was certainly a contemporary of the Teacher of Righteousness. The events of the Nahum Pesher may therefore with some confidence be held to have occurred either during the life of the Teacher of Righteousness or after his death.

It is on every ground improbable that the Nahum Pesher speaks of things that took place before the rise of the Teacher of Righteousness. For the Zadokite Work tells us of a period of twenty years of groping from the first beginnings of the sect to the rise of the Teacher of Righteousness,[4] and it is unlikely that the events here alluded to belonged to that period of groping. If the sect were involved in serious persecution, it would seem to have taken some clear and vigorous line on current affairs, and to be no longer groping, but to have had the leadership of the Teacher of Righteousness.

Demetrius is referred to as one who sought to enter Jerusalem by the counsel of the " seekers after smooth things ".[5] This would seem to refer to the sect's Jewish enemies, and to bring the sharp conflict, in which many of the members of the sect suffered a cruel death, into the time of a Demetrius, and therefore, in view of what has been already said, to locate the period of the Teacher of Righteousness in, or before, the reign of Demetrius. Since this cannot be later than Demetrius III, the period of the Teacher of Righteousness and of the Wicked Priest would seem to fall definitely before the time when the Romans were first seen in Palestine.

This at once reopens the whole question of the identification of the Kittim, and especially the Kittim contemporary with the Teacher of Righteousness mentioned in the Habakkuk Pesher. For if the Teacher of Righteousness lived before the Romans appeared in Palestine, it is difficult to suppose that the references in that Habakkuk Pesher can be to them. Allegro thinks the Wicked Priest and the Kittim need not be located in the same age.[6] This would leave open the identification of the Kittim with the Romans, while requiring the abandonment of the history of the sect and its leader which Dupont-Sommer associated with that identification. There would seem to be little advantage for anyone in this, and it falls foul of the Habakkuk Pesher. For there we read that the wealth of the last priests of Jerusalem would be handed over to the army of the Kittim.[7] The Kittim would therefore appear to be contemporary with the last priests of Jerusalem, and Allegro himself

[1] Col. IX, lines 5 f.
[2] Cf. J. M. Allegro, *J.B.L.*, loc. cit., p. 93.
[3] Ibid., p. 93.
[4] P. 1, lines 9 ff.
[5] Nahum Pesher, line 2.
[6] *J.B.L.*, loc. cit., p. 93.
[7] Col. IX, lines 6 f.

holds that these were contemporary with the Wicked Priest and with the Teacher of Righteousness,[1] who are mentioned in the next sentence of the Habakkuk Pesher.[2]

All of this merely sets the stage for a re-examination of the question of the Kittim. Professor Dupont-Sommer has maintained with much vigour that the term can refer only to the Romans in the Habakkuk Pesher.[3] Since he uncompromisingly holds that the Kittim were active in Palestine in the time of the Wicked Priest, his view here is in flat contradiction to the conclusions Allegro draws from his Nahum Pesher—conclusions which I have already said seem to me to be sound to the extent that they define the latest possible period for the Wicked Priest.

It is common ground that the word Kittim of itself could stand for the Greeks or the Romans.[4] In Dan. xi. 30 it undeniably stands for the Romans. On the other hand, in 1 Macc. i. 1, viii. 5, it equally undeniably stands for the Greeks of the Macedonian empire established by Alexander the Great. On this there is no dispute, and Dupont-Sommer recognizes it as clearly as I. It is also common ground that the word need not necessarily have the same meaning in all the texts of the Dead Sea Scrolls. Dupont-Sommer has argued that whereas in the Battle Scroll it meant the Greeks,[5] it means the Romans in the Habakkuk Pesher. This was because in the Battle Scroll there was a reference to the Kittim of Ashur, and a further reference to the Kittim in Egypt.[6] These were held to be the people of the Seleucid realm and of the Ptolemaic kingdom respectively. More recently, Dupont-Sommer has wavered on this matter, and he now seems inclined to find the reference here also to be to the Romans.[7]

This was because Dr. Yadin, who had seen Allegro's Nahum Pesher, with its reference to "[Deme]trius, king of Yavan", had pointed out that the text

[1] *J.B.L.*, loc. cit., p. 93, where he goes beyond the evidence and argues that the identity of terminology in the Nahum Pesher and the Habakkuk Pesher about the gathering of wealth by the priests in Jerusalem shows that not only were the last priests of the Habakkuk Pesher contemporary with the Wicked Priest, but that the Wicked Priest must be identified with the Lion of Wrath.

[2] Col. IX, lines 9 f.

[3] Cf. *Aperçus préliminaires*, p. 40 (E. Tr., p. 29), and *Nouvelles aperçus*, 1953, pp. 33 ff. (E. Tr. by R. D. Barnett, 1954, pp. 14 ff.).

[4] Cf. what I wrote in *B.J.R.L.*, loc. cit., p. 133, and Dupont-Sommer, *Nouvelles aperçus*, pp. 34 f. (E. Tr. pp. 15 f.).

[5] Cf. *Aperçus préliminaires*, p. 98. (E. Tr., p. 80).

[6] Col. I, lines 2, 4.

[7] Cf. *R.H.R.*, cxlviii, 1955, pp. 42 f. I. Rabinowitz, *V.T.*, iii, 1953, p. 181, had observed that the double identification of the Kittim favoured by Dupont-Sommer was improbable, and Dupont-Sommer dismissed this as "absolument sans valeur" (*Semitica*, v, 1955, p. 53). He would seem now to put some value on it after all, for he now says (*R.H.R.*, loc. cit.) that the interpretation of the Kittim in the Battle Scroll with the Romans "aurait l'avantage d'unifier le sens du mot Kittim dans l'ensemble des documents de Qoumrân". It would appear that a single identification is valueless if it is not the Roman identification, but advantageous if it is !

did not say " King of the Kittim of Ashur ", and that therefore the Kittim were to be differentiated from the Greeks.[1] This is quite unconvincing, since in 1 Macc. i. 1 it is said that Alexander the Great came from the land of the Kittim, and became ruler over Greece, which must correspond to Yavan in the original text.[2] It is clear that in this verse Kittim stands for the Greek or Macedonian people, while Greece, or Yavan, stands for the empire established by Alexander.[3] The two terms are found in a single verse here, and are clearly defined in meaning. We cannot rule out the possibility, therefore, that the Scrolls similarly use Yavan for the Macedonian empire, and Kittim for the Greek or Macedonian people. It should be remembered that the rulers of the Hellenistic kingdoms maintained the fiction that the empire of Alexander was still one and undivided, and they were all rulers of that empire.[4] In fact the Nahum Pesher contains a reference to the " rulers of the Kittim " as well as to "[Deme]trius, king of Yavan ", and in view of 1 Macc. i. 1 we cannot simply assume that the one has reference to the Greeks and the other to the Romans. Much has been made of the fact that the Habakkuk Pesher speaks of the " rulers of the Kittim ", and Dupont-Sommer has argued that the reference could not be to the Seleucids, or the term " king " would have been used.[5] He therefore maintains that the reference is to the Roman commanders,[6] where " rulers " would be appropriate, but " king " would not. Alas for his new-found readiness to find the Romans in the Kittim of the Battle Scroll ! In that text we find a reference to the " king of the Kittim ",[7] which would certainly not be appropriate to the Romans, at any rate before Augustus. Whatever happens to the Kittim of the Habakkuk Pesher, those of the Battle Scroll will have to be recognized to be Greeks or Macedonians.

[1] In *Ha-aretz*, July 23rd, 1955, cited by Dupont-Sommer, loc. cit. I have not had access to this article.

[2] Cf. F.-M. Abel, *Les livres des Maccabeés*, 1949, p. 2.

[3] Cf. Abel, ibid.

[4] Cf. E. Bevan, *The House of Seleucus*, i, 1902, pp. 57 f.: " These kings are never officially styled kings *of Egypt* or kings *of Asia*. If they are so called by historians, it is merely for the purpose of convenient distinction. It connoted rather a personal relation to the Macedonian people. *Ideally* there was one Macedonian Empire as in the Middle Ages there was one Roman Empire. But the dignity of Macedonian King was borne conjointly or concurrently by several chieftans, just as the dignity of Roman Emperor was borne concurrently by the Western and the Byzantine prince. *In practice*, of course, each of the rivals had to acquiesce in the others being kings within a certain territorial sphere . . . Ptolemy and Seleucid were to the end Macedonian kings who happened to reign in Egypt and in Asia ". Cf. also B. Niese, *Geschichte der griechischen und makedonischen Staaten seit der Schlacht bei Chaeronea*, ii, 1899, p. 123: " Das Reich Alexanders ist trotz den Teilungen immer noch als ein Ganzes anzusehen . . . Jeder der neuen Könige hielt sich berechtigt, nach Vermögen und Gelegenheit seinen Teil zu vergrössern, ja selbst das Ganze in Anspruch zu nehmen ".

[5] Cf. *Nouveaux aperçus*, pp. 38 f. (E. Tr., p. 19).

[6] Cf. *Aperçus préliminaires*, p. 41 n. (E. Tr., p. 30 n.), and *Nouvelles aperçus*, pp. 38 f. (E Tr., pp. 18 f.).

[7] Col. XV, line 2.

It may be added that if the suggestion that the term "rulers of the Kittim" meant military commanders were accepted, it would surely be equally possible that it might stand for the governors or generals active in Palestine under the Seleucid monarchs.[1] In the Nahum Pesher it is possible that it stands for the upstarts and pretenders to the Seleucid throne, who clutter the history of the second century B.C. The phrase in the Nahum Pesher is "from Antiochus to the rise of the rulers of the Kittim", and Allegro thinks this covers a period of about 120 years from Antiochus Epiphanes to the appearance of the Romans in Jerusalem, running back about a century before the time of the supposed martyrdom of the Teacher of Righteousness. This seems to me improbable, especially as the gap in the text is very slight, and the period of time seems to be associated with the "seekers after smooth things". If these were the Jewish enemies of the sect, a much shorter period of time would seem more probable. In view of the obscurity of the text, however, no certainty is attainable.

Dupont-Sommer dismisses Antiochus from the Habakkuk Pesher by observing that the text states that the Kittim arrived from afar, from the islands of the sea.[2] This, he says, would be perfectly relevant to the Romans, but quite irrelevant to the armies of Antiochus Epiphanes. It is a curious fact, as pointed out by Stauffer[3] and Michel,[4] that 1 Macc. vi. 28 says that when Antiochus Eupator was angry at the news of the activity of Judas Maccabæus, he gathered together an army, in which were included men from other kingdoms, and from "the isles of the sea", while Josephus says the army of Antiochus Epiphanes included mercenaries from the islands.[5] Later we find that Demetrius I is said to have raised troops to attack the Jews from "the isles of the Gentiles".[6] There is therefore no validity in this argument.[7]

It is to the worship of "signs", however, that Dupont-Sommer appeals as the sufficient *coup de-grâce* to any other view than the Roman. In the Habakkuk Pesher the Kittim are said to have sacrificed to their standards and to have worshipped their arms.[8] This, Dupont-Sommer maintains, is

[1] R. Goossens objects to this that the Seleucid governors and generals derived their authority from the king, and hence the term would be more appropriate to the Roman pro-consuls, pro-prætors and pro-magistrates (*La Nouvelle Clio*, iv, 1952, pp. 142 f.). But did not these derive their authority from the Senate ?

[2] Cf. *Aperçus préliminaires*, p. 41 (E. Tr., p. 29), *Nouvelles aperçus*, pp. 35 f. (E. Tr., pp. 16 f.); also C. Detaye, *E.Th.L.*, xxx, 1954, p. 324.

[3] Cf. *Th.L.Z.*, lxxvi, 1951, col. 671.

[4] Cf. *Le Maître de Justice*, 1954, pp. 135 f.

[5] Cf. *Antiq.*, XII, vii. 2 (xii. 293).

[6] 1 Macc. xi. 38. Cf. Josephus, *Antiq.*, XIII, iv. 9 (xiii. 129), where Crete is also specified.

[7] R. Goossens, loc. cit., p. 142, cites the evidence from 1 Maccabees, and admits the weakness of this argument; but though his article is much used by both Dupont-Sommer and Detaye, they repeat the argument and ignore the evidence against them.

[8] Col. VI, lines 4 f.

a clear reference to the Roman cult of standards.[1] The term used in the Habakkuk Pesher for standards is " signs ", and the Latin term for standards is *signa*.

In my book *The Zadokite Fragments and the Dead Sea Scrolls* I noted that according to Brown-Driver-Briggs the word *'ôth* is used in Psa. lxxiv. 4 with the meaning " standard ", and that in that case the psalm refers to the placing of enemy standards in the Temple, and so brings them into association with worship.[2]

Psa. lxxiv has long been thought to come from the Maccabæan period. Dupont-Sommer is here not quite fair to his opponents when he says that to escape from an untenable position they invoke Psa. lxxiv, which they freely (*volontiers*) date in the Maccabæan age.[3] This quite falsely suggests that this dating is something that has been thought up to bolster a theory about the Scrolls. In fact it was not propounded in the interests of any theory on the Scrolls, or by anyone who had an axe to grind. It was put forward many decades—and even centuries—before the Scrolls were heard of, and on quite independent grounds, and it has been accepted by a long line of scholars.[4] These were not merely scholars who had a *penchant* for bringing psalms down to the Maccabæan age, but sober and relatively conservative scholars, such as Delitzsch,[5] Baethgen,[6] and Kittel,[7] while so cautious a scholar as Calès thought the psalm had been revised in the Maccabæan period.[8] It is to be noted that in the psalm there is a reference not merely to the desecration of the Temple, but to the burning of the " meeting places of God " throughout the land,[9] and the lament that there is no longer a prophet[10]—precisely as we find in 1 Macc. iv. 46, ix. 27, xiv. 41. I should add that in my book above referred to I did not pin myself to the Maccabæan dating of this psalm, but observed that if it was not from the Maccabæan period it was certainly older, and that therefore we have in

[1] Cf. *Aperçus préliminaires*, p. 42 (E. Tr., p. 31), *Nouvelles aperçus*, pp. 39 ff. (E. Tr., pp. 20 ff.); also Goossens, loc. cit., pp. 146 ff., and Detaye, loc. cit., pp. 324 ff.

[2] Cf. *The Zadokite Fragments and the Dead Sea Scrolls*, p. 73.

[3] Cf. *Nouvelles aperçus*, p. 43 (E. Tr., p. 23).

[4] Cf. *The Zadokite Fragments and the Dead Sea Scrolls*, p. 74 n., where I give references to a number of scholars, of whom the earliest is Hitzig, *Die Psalmen*, ii, 1865, pp. 130 ff. But Hitzig did not originate the idea. Already in Poole's *Synopsis criticorum*, ii, 1684, col. 1044, there is mention of the view that the psalm was a prophecy of the time of Antiochus, and Calvin favoured this view (cf. *A Commentary on the Psalms of David*, E. Tr. (Anon.), ii, 1840, p. 290).

[5] Cf. *Biblical Commentary on the Psalms*, E. Tr., by D. Eaton, ii, 1888, p. 377.

[6] Cf. *Die Psalmen*, 1897, pp. 227 f.

[7] Cf. *Die Psalmen*, 5th ed., 1929, p. 250.

[8] Cf. *Le livre des Psaumes*, ii, 1936, pp. 18 f. P. C. Boylan, *The Psalms*, ii, 2nd imp., 1931, p. 9, says that the Maccabæan dating of the psalm is the one modern Catholic scholars incline to.

[9] Psa. lxxiv. 8.

[10] Psa. lxxiv. 9.

the Bible a reference to standards in association with worship long before the Roman period.[1]

Professor Dupont-Sommer therefore expresses doubt whether the word can mean " standards " in the psalm, and dismisses it as unintelligible, dubbing the argument from this psalm as " perfunctory " (*par trop sommaire*).[2] I would repeat that it was not in the interests of any theory about the Kittim that it was claimed that the word meant " standards " in this passage. The editors of the *Oxford Hebrew Lexicon*[3] can hardly be credited with sufficient prophetic insight to anticipate the finding of the Habakkuk Pesher, and still less can their predecessors in this understanding of the word.[4] It may be recalled that Athanasius understood the word to have this meaning in the psalm.[4a] It is therefore clear that this interpretation is completely objective, and not based on any theory about the Scrolls, and it has been favoured by scholars of such unimpeachable integrity as C. A. Briggs.[5]

Dupont-Sommer rightly observes that in this same psalm the word *'ôth* is used in a sense other than " standards ", with reference to the religious symbols of the Jews.[6] This was already obvious to those who found the meaning " standard " in the verse referred to. But Dupont-Sommer, who finds the reference to standards so sure in the Habakkuk Pesher, would surely not claim that to the Jews of the period when that *pesher* was written the word *'ôth* had only the meaning of " standard ", and could not be used in any other sense. If a word could admittedly have more than one sense—as many words in all languages have, including, for that matter, the Latin *signa*—it is obvious that an author must be allowed to use it in all its senses. The English work " know " corresponds to the French " savoir " and " connaître ". Would Professor Dupont-Sommer deny me the right to say " I know Professor Dupont-Sommer and I know what he has written" ?

That the reference could be to the Roman cult of *signa* in the period to which Dupont-Sommer assigned the Habakkuk Pesher is less certain than he claims, and with the pushing back of the date of the Teacher of Righteousness to the time of Jannæus or earlier, it becomes less sure still. H. J. Rose—

[1] Cf. *The Zakodite Fragments and the Dead Sea Scrolls*, p. 75. I would add that I am increasingly doubtful if there are any Maccabæan psalms in the Psalter.

[2] Cf. *Nouvelles aperçus*, p. 43 (E. Tr., p. 23).

[3] Cf. *A Hebrew and English Lexicon of the Old Testament*, 1907, p. 17 a.

[4] Again I can point to Poole's *Synopsis*, ii, col. 1046, and Calvin, op. cit., ii, p. 293, for evidence of this understanding of the word centuries ago. It may be added that A. F. Kirkpatrick, *The Book of Psalms*, 1906 ed., p. 44, though he did not accept the Maccabæan dating of the psalm, accepted this meaning of the word *'ôth*, and the translators of the Authorized Version appear to have done the same.

[4a] Cf. Migne, *P.G.*, xxvii, 1886, col. 333: *Ἔθεντο τὰ σημεῖα αὐτῶν σημεῖα. Τὰ σύμβολά φησι τοῦ πολέμου· λέγει δὲ τὰ καλούμενα παρὰ τοῖς στρατεύμασι σίγνα.*

[5] Briggs was responsible for the relevant entry in the Oxford Hebrew Lexicon.

[6] Cf. *Nouvelles aperçus*, pp. 43 f. (E. Tr., p. 23).

again with no axe to grind in the present connexion—says in the *Oxford Classical Dictionary* that our information about the cult of *signa* does not go back to Republican times.[1] In imperial times we know that there was veneration of standards, and Josephus records[2] that at the time of the Fall of Jerusalem in A.D. 70 the Romans carried their standards (σημεῖα) into the Temple court and sacrificed to them. H. St.J. Thackeray in 1928—again with no axe to grind—said that for this practice Josephus seems to be the sole authority.[3] In the Habakkuk Pesher the Kittim are said to sacrifice to their signs (*'ôthôth*).[4]

Goossens claimed to find evidence of this cult in Republican times,[5] and Dupont-Sommer makes much of it, and expresses some scorn[6] for the doubts I had on the subject.[7] I cannot claim to be a classical scholar, and must leave to others the fuller examination of this question; but it may not be out of place to look at it in the light of the help afforded by classical authorities. In *In Catilinam* Cicero says: " I know that you have arranged and appointed a day with Manlius and that you have also sent forward that silver eagle (*aquilam illam argenteam*) . . . For this eagle a shrine of iniquities has been set up in your own home ".[8] In my already mentioned book I observed—and I still think the observation was relevant, and am glad to have the support

[1] Cf. *The Oxford Classical Dictionary*, 1949, p. 857 b. Professor Dupont-Sommer says that my suggestion that the Roman worship of *signa* arose only in the Imperial period was made in the interests of my argument. In view of the fact that it was not my suggestion but rested on a precise statement by H. J. Rose, which I cited, and which was made quite independently of my argument, this is patently inaccurate. It is quite certain that Professor Rose's statement was made without any knowledge of the Habakkuk Pesher, or reference to it.

[2] Cf. *B.J.*, VI, vi. 1 (vi. 316).

[3] Cf. Loeb *Josephus*, iii, 1928, p. 469 n.

[4] Col. VI, line 4.

[5] Professor Goossens very courteously brought this to my notice privately, as I acknowledge in *The Zadokite Fragments and the Dead Sea Scrolls*, p. 73 n., and later published it *Le Flambeau*, xxxv, 1952, pp. 152ff. He returns to it in *La Nouvelle Clio*, loc. cit., pp. 151 ff.

[6] Cf. *Nouvelles aperçus*, p. 47 n. (E. Tr., p. 25 n.). Dupont-Sommer observes that in the army of Germanicus the standard was in the actual house of the General, and asks me " Was this standard therefore the object of a private cult ? " To this quite irrelevant question I have no hesitation in replying " No ". I would add that still less is it any evidence of a public cult, such as Dupont-Sommer postulates for the standards in Catiline's private chapel. Indeed, there is no evidence whatever of the worship of the standard in the incident involving Germanicus referred to. Tacitus describes (*Annals*, I, xxxix) how Germanicus was forced to hand over the colours (*vexillum*), which were kept in his quarters. J. Jackson comments: " The veterans . . . demand their *vexillum* as the symbol and guarantee of their status, in case the motives of the deputation should prove sinister " (Loeb edition of Tacitus, ii, 1943, p. 310 n.). This has nothing whatever to do with the *worship* of *signa*, and still less with *sacrifice* to *signa*.

[7] Loc. cit.

[8] *In Catilinam*, I, ix. 24 (E. Tr. by Lord, in Loeb ed., Cicero, *The Speeches*, 1937, p. 37).

of Professor Driver[1]—that this offered no evidence of the worship of standards by armies, since there is a reference only to the private chapel of Catiline.[2] I may add that it affords no evidence of any sacrifice to the standard. I may further add that the word *signa* is not used here, but a silver eagle. H. M. D Parker says that " under the Principate the legion retained its *aquila* and *signa* ", while " the prætorians had only *signa* and *vexilla* ".[3] This would seem to differentiate the *aquila* from the *signa*, though both fell within the general category of standards. And so does Cicero. For later he says: " When he was trapped and held fast, I inquired why he hesitated to go where he had long intended going, when already arms, axes, the fasces, the trumpets, the military standards (*signa militaria*), and that silver eagle for which he had prepared a shrine of crime in his own home had been sent in advance ".[4] This clearly distinguishes between the *aquila* and the *signa*, and the passage offers no evidence of sacrificing to signs[5] comparable with the Josephus passage, where the word used is σημεία.

I may observe in passing that whereas Dupont-Sommer dismisses Psa. lxxiv since at best it could offer evidence only that standards were brought into the Temple and not worshipped, he is far less exacting of the evidence he presses into his service. He is prepared to accept as evidence for sacrifice to *signa* by Roman armies in Republican times evidence which makes no mention of the armies, of sacrifice, or even of *signa*. It would be unkind if I borrowed his expression and described this as " perfunctory ".

I may cite another classical authority for the view that the religious treatment of standards was different in Republican Rome from that which they received under the Empire. Writing nearly half a century ago in Daremberg and Saglio's *Dictionary*, A. J. Reinach said that whereas under the Republic the standards were placed, at the end of each campaign, in the *aerarium* of the sanctuary of Saturn and of Ops, under the Empire each

[1] Cf. *J.O.R.*, N.S. xliv, 1953-54, p. 4, where Driver observes: " These words, as also the description of this shrine in Catiline's house as *sacrarium scelerum*, show clearly that it is no official cult of legionary emblems that is being abused; if it had been, the charge would have described it explicitly as such. In fact, the eagle was that which Marius had had in the Cimbrian war in 101 B.C.E. (Sallust, *Catilina*, 59). This, though originally treasured as a symbol of Rome's military power, had become an object of veneration not for that reason, but as having belonged to the great soldier who was regarded as the prototype of a democratic leader; it was not a military but a revolutionary cult which this eagle symbolized ".

[2] Loc. cit.

[3] Cf. *The Oxford Classical Dictionary*, p. 838a.

[4] *In Catilinam*, II, vi. 13 (E. Tr. by Lord, op. cit., p. 61).

[5] It is curious to observe that whereas Dupont-Sommer dismisses Rabinowitz's suggestion that the Hellenistic armies offered divinatory sacrifices in the vicinity of their pennons (*V.T.*, iii, 1953, p. 181 n.) by remarking that what is in question is sacrificing to the standards themselves (ibid., v, 1955, pp. 115 f. n., and *Semitica*, v. 1955, p. 54), he draws no attention to the absence of any mention of sacrifice to Catiline's standard. It would seem that he excuses himself from the hard conditions of rigorous proof which he would impose on others.

camp had to have a chapel for the standards, and here the images of the Emperors were also worshipped.[1] This brings the cult of *signa* by armies into association with the ruler cult, and implies that we cannot read back into Republican times the practice under the Empire. When it is remembered that Josephus tells us that the standards which Pilate brought into Jerusalem bore the effigy of Caesar,[2] it would seem that the sacrifice to standards in Jerusalem at the time of the Fall of Jerusalem was closely related to the ruler cult. No evidence of sacrifice to *signa* by Republican Roman armies has yet been adduced.

We may turn to ask whether there is evidence which points to any practice in Seleucid Syria which could explain the reference in the Habakkuk Pesher. I had observed that in the absence of any information as to the nature of the Seleucid standards, we could not rule out the possibility that they bore the emblems of Zeus.[3] To this Dupont-Sommer replies somewhat tartly that such a possibility is not a proof that they did.[4] I had not claimed that it was such a proof, and it would be open to me to reply that Dupont-Sommer's ignorance of the standards of the Seleucids is equally no proof as to their character. As Stauffer observes, for this period Papyri are lacking and there is a paucity of *Kunstdenkmäler*.[5] In the absence of direct evidence either possibility should be kept open, and that was all that I asked. I may add that there is evidence of ruler cult under Antiochus Epiphanes,[6] just as there is for Imperial Rome, when alone the practice of sacrificing to *signa* by Roman armies is attested.

Delcor had said that the worship of standards was " monnaie courant " in the east, and that the practice was common among the Assyrians, Persians

[1] Cf. Daremberg and Saglio, *Dictionnaire des Antiquités*, IV 2, 1908-11, p. 1324 b: "Sous la République, on devait ramener, après chaque campagne, les enseignes dans l'*aerarium* du sanctuaire de Saturne et d'Ops, ou les questeurs allaient prendre et porter cette partie du trésor public . . . Sous l'Empire, les temples de Rome continuèrent à recevoir des *signa*; mais il fallut que, dans chaque camp, dressé suivant les règles religieuses du *templum*, les enseignes eussent leur chapelle, refuge inviolable, où étaient aussi adorées les *imagines* des empereurs". It may be observed that the incident referred to above is clear evidence that the standards were not placed in a chapel set up in the camp in the time of Germanicus.

[2] *Antiq.*, XVIII, iii. 1 (xviii. 55).

[3] Cf. op. cit., p. 73.

[4] Cf. *Nouvelles aperçus*, p. 43 (E. Tr., p. 23).

[5] Cf. *Th.L.Z.*, loc. cit., col. 672 n.

[6] Cf. E. Bevan, *The House of Seleucus*, ii, 1902, pp. 154 ff. Bevan says that this cult goes back to Alexander, and cites documentary evidence for it in the time of Seleucus IV (p. 154 n.). He adds: " But undoubtedly Antiochus IV lays more stress upon his deity than former kings " (p. 154), and again: " The God-King gave a fixed object of worship among the chaos of local cults". On ruler cult in the Hellenistic kingdoms, cf. U. Wilcken, *Sitzungsberichte der Berliner Akademie der Wissenschaften*, 1938, pp. 298 ff; also L. R. Taylor, *J.H.S.*, xlvii, 1927, pp. 53 ff., A. D. Nock, ibid., xlviii, 1928, pp. 21 ff., W. W. Tarn, ibid, pp. 206 ff., M. Launey, *Recherches sur les armées hellénistiques*, ii, 1950, pp. 945 ff., and Herzog-Hauser, in Pauly-Wissowa, *R.E.*, Supplement, ii 2, cols. 376 f.

and Egyptians.[1] To this Dupont-Sommer replies that the observation is irrelevant, since only Seleucid and Roman practice is in question.[2] It is at least relevant as evidence that such worship was not confined to Roman armies, and in view of the known practice of the Hellenistic kingdoms to assimilate much of oriental origin, it strengthens the possibility that such a practice was found in Seleucid Syria.

There is more than this, however, to be said. When I wrote in 1952 I was unaware of an article published by a Parisian professor in *Zalmoxis* in 1938[3]—and therefore long before the discovery of the Scrolls, and without any axe to grind on the present issues—in which it was argued that the worship of standards prevailed in the east before Roman times. The article appeared in a journal which is very difficult of access, and it treated of the cult of standards amongst the Scythians and in India.[4] So far as India is concerned I may note that the evidence is from post-Alexandrian times, and bears evidence of a fusion of Greek and oriental ideas.[5]

More relevant to our present purpose is its citation of du Mesnil du Buisson's evidence[6] that in several temples of Dura-Europos there was a perforated stone which served to keep a standard erect.[7] It is then noted that on Persian coins of the second century B.C. a standard is planted on the right of the sanctuary, and that it is often surmounted by a bird.[8] Du Mesnil du Buisson recalls [8a] that in Lucian's *De Dea Syra* we are told that in a temple at Hierapolis in northern Syria an idol of gold stood between the statues of Hera and Zeus (Atargatis and Hadad), and that this was different from the other idols. It had no special form of its own, and was known by the Syrians as a sign (*σημήϊον*).[9] The word used here is the Ionic form of the word Josephus used for "standard" in the above cited passage, and it corresponds precisely to the Hebrew *'ôth*. Lucian says that it is taken down to the sea twice a year to bring up the water which is poured out in the temple.[10] In the article in *Zalmoxis* it is added that there is evidence from Dura-Europos which links up with that concerning Hierapolis, and moreover

[1] Cf. *Essai sur le Midrash d'Habacuc*, 1951, p. 42 n.; also *R.B.*, lviii, 1951, p. 527. On the deification of standards in Egypt, cf. H. Frankfort, *Kingship and the Gods*, 1948, pp. 88, 91 ff. For Assyria cf. R. Labat, *Le caractère religieux de la royauté assyro-babylonienne*, 1939, pp. 259 ff., cap. p 260 : "Ces enseignes étaient vénérées, à l'instar des statues divines, comme la personnification même du dieu.. Dans les camps, on leur offrait des sacrifices".

[2] Cf. *Nouvelles aperçus*, p. 41 n. (E. Tr., p. 21 n.).

[3] Cf. J. Przyluski, *Zalmoxis*, i, 1938, pp. 13 ff.

[4] The title of the article is "Le Culte de l'Étendard chez les Scythes et dans l'Inde".

[5] Cf. *Zalmoxis*, loc. cit., p. 14.

[6] Cf. *Revue des Arts Asiatiques*, xi, 1937, pp. 75 ff.

[7] Cf. *Zalmoxis*, loc. cit., p. 15.

[8] Ibid, p. 14.

[8a] Loc. cit., pp. 76 ff.

[9] Cf. *De Dea Syra*, 33. Cf. C. Clemen, *Lukians Schrift über die syrische Göttin*, 1938, p. 21.

[10] Cf. *De Dea Syra*, loc. cit.

shows that a portable altar there was dedicated to the standard.[1] Of itself the evidence from Dura-Europos would be too late to be of service, but its link with Syrian practice elsewhere and with other oriental practice of an earlier date gives it significance for us. Du Mesnil du Buisson says it is not necessary to suppose that the cultic emblems of the Asiatic gods derived from the Roman standards. The divine standards associated with the cult of the gods in the religions of the East are an institution of very great antiquity.[2]

Writing of Hierapolis in 1914—and therefore again without *parti pris* on the Scrolls—A. B. Cook said that " on the whole it seems probable that a royal sceptre or standard, enclosed in a shrine of its own, was *the central object of worship* ".[3] He adds: " Garstang conjectures that this cult-object was originally a pillar-altar with a pigeon or dove upon it . . . later conventionalized into a Roman standard in an *ædicula* ".[4] Here, then, are scholars who, without reference to the Scrolls, have argued that the cult of standards is of ancient oriental origin, and that it was continued in Syria into post-Christian times, and that this oriental cult was the source of the Roman cult, just as the ruler-worship with which it was associated in Imperial Rome was of oriental origin.

It seems to me that the evidence of Syrian worship of *signs* is stronger than anything that has been produced from pre-imperial Rome,[5] even though it falls short of an absolute demonstration. When it is remembered that it was the policy of the Macedonian kingdoms to mingle the culture of Greece with oriental culture, and that there is undisputed evidence of a wider background of the worship of standards in the East,[6] it seems to me clear that we should keep open the possibility that the Habakkuk Pesher refers to Seleucid

[1] Cf. *Zalmoxis*, loc. cit., p. 15. Cf. also *Revue des Arts Asiatiques*, loc. cit., pp. 80 f.

[2] Ibid., p. 75.

[3] Cf. *Zeus*, i, 1914, p. 587.

[4] Ibid., n. 4. For the reference to Garstang, cf. Strong and Garstang, *The Syrian Goddess*, 1913, pp. 23 ff.

[5] It may also be added that Przyluski notes that the Scythians used to offer sacrifices to a short sword, which was set up as the symbol of the god, and finds here an ancient form of standard. He observes that the cult of standards was perfectly adapted to the habits of nomad warriors like the Scythians (loc. cit., p. 18). We have no need to look to Rome, then, for either the cult of standards, or the worship of arms, both of which are mentioned in the Habakkuk Pesher, and Przyluski observes that Roman standards developed under the influence of oriental religions (ibid., p. 17). Cf. also F. Sarre, *Klio*, iii, 1903, p. 370: " Das Feldzeichen trägt also nicht nur in seiner allgemeinen Form, sondern auch in Rücksicht auf die Bedeutung seiner Darstellungen den Charakter einer *rein orientalischen Schöpfung* ".

[6] Cf. H. Schäfer, *Klio*, vi, 1906, p. 396, where, in an article on " Assyrische und ägyptische Feldzeichen ", we read: " Sind die Feldzeichen Götterbilder, so ist es nur selbstverständlich, dass sie auch einen richtigen Kult geniessen. In der Tat ist uns das ja auf einigen assyrischen Reliefs wirklich dargestellt ". An illustration of an Assyrian camp, where an offering is being made before the chariots which carried the standards, is given.

practice and not necessarily to Roman. It is less justified to antedate by more than a century the sacrifice to standards by Roman soldiers, which is only attested by Josephus, and only for A.D. 70, than to accept the probability that an ancient oriental practice has been carried on in the East. And I would emphasise that I have only cited the views of scholars who wrote before the discovery of the Scrolls, whereas on the other side no scholar who wrote before the publication of the Scrolls has been cited for the view that the Roman practice of sacrifice to their standards prevailed in the Republican period.

Let me then briefly summarize my argument: (1) the evidence of the Nahum Pesher points clearly to a pre-Roman period for the life and work of the Teacher of Righteousness; (2) the claim to find the worship of standards amongst the armies of Republican Rome is at best doubtful; (3) the evidence for the worship of standards in Syria before the coming of the Romans, as well as more widely in the ancient East, is, to say the least, stronger.

This still leaves open the question whether the Teacher of Righteousness belonged to the period of Alexander Jannæus or to that of Antiochus Epiphanes, and I must briefly touch on this in conclusion.

The period of Jannæus seems to me unsuitable for a variety of reasons. Jannæus is known, indeed, to have crucified large numbers of his enemies,[1] and as has been said, the Nahum Pesher probably speaks of one who crucified members of the sect. Allegro assumes that after he had crucified those who opposed him in battle, Jannæus turned on the sect, and "stormed down" to Qumran to crucify the Teacher.[2] This is a curious assumption to make on the basis of a text which does not so much as mention the Teacher! Moreover, we are offered no clear reason why Jannæus should turn on the sect at this time.

In the time of Jannæus many Jews invoked the aid of Demetrius III against their king, whose anger against these, when he had them in his power, is to be understood.[3] It is known that some of those who first sought the help of Demetrius later turned over to the side of Jannæus,[4] and Allegro

[1] Cf. Josephus, *Antiq.*, XIII, xiv. 2 (xiii. 380), *B.J.*, I, iv. 6 (i. 97).

[2] Cf. *Time* magazine (New York), February 6th, 1956, p. 37, and S. Zeitlin, *The Dead Sea Scrolls and Modern Scholarship*, 1956, p. xi (where Allegro's own words are cited).

[3] Cf. Josephus, *Antiq.*, XIII, xiv. 2 (xiii. 379 f.), *B.J.*, I, iv. 6 (i. 96 f.).

[4] Cf. Josephus, *Antiq.*, XIII, xiv. 2 (xiii. 379), *B.J.*, I, iv. 5 (i. 95).

appears[1] to think that the members of the sect were amongst these, though he says that they seem to have been as much opposed to Demetrius as to Jannæus.[2] In fact, there is no evidence at all that the sect invoked the aid of Demetrius. The Nahum Pesher speaks of the " seekers after smooth things " who were on the side of Demetrius,[3] and it is clear that the sect from which the Nahum Pesher came did not mean themselves by this offensive term. It is gratuitious to assume that they were at first the allies of the " seekers after smooth things ", and then turned against them. It is equally gratuitous to assume that Jannæus, after crucifying those who had fought against him, turned to crucify those who had saved the position for him by their defection to him, when we have no hint of such a thing from any source, and still more gratuitous to suppose that we have here any shadow of evidence of the crucifixion of the unmentioned Teacher of Righteousness.

In the time of Jannæus, Demetrius was his enemy. If Jannæus were the Wicked Priest, therefore, and the Seleucids were the Kittim, these were not allies but foes. Yet the Habakkuk Pesher strongly suggests that the Wicked

[1] Allegro is so vague and imprecise that it is not always easy to perceive what his view is. But since he finds in the conflict between Jannæus and Demetrius the concrete historical situation which led to the death of the Teacher of Righteousness (*J.B.L.*, loc. cit., pp. 92 f.), he would seem to assign to the sect some part in that conflict. Moreover, since he links the supposed crucifixion of the Teacher of Righteousness directly with the crucifixion of the eight hundred Pharisees, he would appear to bring the sect into connection with the events which led up to that crucifixion. In his letter to *The Times* on March 20th, 1956, he says that from the fact that Jannæus crucified eight hundred Pharisees it is a not unreasonable inference that he also crucified the Teacher of Righteousness. This is in no sense a reasonable or a relevant inference unless the Teacher of Righteousness were in some way associated with the events which provoked the crucifixions.

[Mr. Allegro now informs me privately that he does not connect Jannæus's persecution of the Pharisees with an attack upon the sect, though he continues to infer that the Teacher of Righteousness was crucified. I presumed that he did postulate some connection, in order to provide him with a bridge, however flimsy, to get from the crucifixion of the Pharisees to his supposed crucifixion of the Teacher of Righteousness. If he repudiates that bridge, he has nothing more than intuition to offer to justify his leap from the crucifixion of the eight hundred Pharisees to the crucifixion of the Teacher of Righteousness, which he says is a legitimate inference from it. I did not suspect that he could be guilty of such levity. Any objective study of the texts demands that similar evidence should be similarly interpreted. If, as Allegro holds, it is a legitimate inference from the fact that the Nahum Pesher records the crucifixions that the Teacher of Righteousness was also crucified at about the same time, then it is equally legitimate to infer from the fact that the Habakkuk Pesher records that the Kittim caused many to perish by the sword that the Teacher of Righteousness was killed again many years later by the Kittim ! For Allegro ascribes the first appearance of the Kittim on the scene to a time long after the crucifixion of the Teacher of Righteousness. If we are asked to interpret these two similar pieces of evidence in two totally different ways on the simple authority of Allegro's *ipse dixit*, we are asked to abandon any objective study of the Scrolls for a purely arbitrary exegesis that is dictated by Allegro's theory.]

[2] Cf. *J.B.L.*, loc. cit., p. 92.

[3] Line 2;

Priest and the Kittim were on the same side against the sect.[1] If, on the other hand, as Allegro holds, Jannæus was the Wicked Priest, and the Kittim were the Romans,[2] who first came on the scene some years after the death of the Teacher of Righteousness, and after the death of Jannæus, we are left without any explanation of the mixing up of the Wicked Priest and the Kittim. Allegro thinks that the Romans (Kittim) were the foreign foes, as the Wicked Priest was the Jewish enemy of the sect. Yet he offers no evidence that the Romans persecuted members of the sect, and so earned their hostility. Dupont-Sommer located the death of the Teacher of Righteousness a few years before Pompey's arrival in Jerusalem,[3] and so brought his Kittim into active relations with his Wicked Priest, though not with members of the sect.[4] Allegro holds—I think rightly—that the mention of the crucifixions in the Nahum Pesher implies that these had some particular relevance for the history of the sect,[5] and he endeavours to provide a historical background for this in the time of Jannæus. The Habakkuk Pesher says the Kittim caused many to perish by the sword, and had no mercy on the fruit of the womb.[6] This, by the same token, should imply that members of the sect suffered at the hands of the Kittim. What new background of conflict with the Romans is to be postulated for the sect many years after the death of the Teacher of Righteousness ?

We may now turn to the period of Antiochus Epiphanes, and see how relevant the position then becomes. If the Teacher of Righteousness lived at that time, he lived in an age for which we have the fullest evidence that Jerusalem priests, including High Priests, were hand in glove with the Seleucid authorities, and that against both were groups of Jews who were fanatically devoted to the Law.[7] The conditions of the Habakkuk Pesher would be fully satisfied. Moreover, the conditions of the Nahum Pesher would be equally met. For there were crucifixions of Jews by the agents of Antiochus in that age, which could just as appropriately be referred to by

[1] Both the Wicked Priest and the Kittim were foes of the sect (see below), and the reference to the passing of the wealth of the priests to the Kittim suggests that these were contemporary foes. They were therefore more likely to be allied foes than quite independently hostile, since foreign foes were not likely to be independently acting against dissident Jewish groups unless they were the allies of the Jewish authorities against whom they were active.

[2] Cf. *J.B.L.*, loc. cit., p. 93. Allegro avoids all problems by the simple statement that the Kittim were " clearly " the Romans.

[3] Cf. *Aperçus préliminaires*, 1950, p. 47 (E. Tr., p. 35).

[4] It must be remembered that Dupont-Sommer identified the Kittim of the Battle Scroll with the Greeks or Macedonians, and the bitter hostility of the sect to them would in that case be quite distinct from any feelings towards the Kittim of the Habakkuk Pesher, whom he identifies with the Romans. He interprets the references to the Kittim in the Habakkuk Pesher quite generally, and without any reference to the sect or its history.

[5] Cf. his Letter to *The Times*, March 20th, 1956.

[6] Col. VI, lines 10 ff.

[7] Cf. 1 Macc. i. 11 ff., vii. 1 ff.

the words of the Nahum Pesher as those of Jannæus. Josephus says: " They were whipped, their bodies were mutilated, and while they were still alive and breathing, they were crucified, while their wives and the sons whom they had circumcised in despite of the king's wishes were strangled, the children being made to hang from the necks of their crucified parents ".[1] Surely this would amply justify the statements of both the Nahum Pesher and the Habakkuk Pesher, and there is no need to assume two separate persecutions, one by the Wicked Priest, and one by the Kittim long after he was dead. In the age of Antiochus, whoever was against the Wicked Priest was against the Kittim, or Seleucids, and whoever was persecuted by the one would be persecuted by the other. A single situation suffices to explain all.

Further, the mention of Demetrius, Antiochus, and the crucifixions within three or four lines in the Nahum Pesher would be very natural. Antiochus died in 164 B.C. We know that in 161 B.C. Demetrius I sent his troops to Jerusalem, with the support of the pro-Seleucid faction, and that Nicanor confidently expected to get the Temple back from the control of Judas and his friends.[2] So far as Demetrius III is concerned, Allegro admits that we have no evidence from any other source that he sought to enter Jerusalem.[3] So far as Demetrius I is concerned, we have such evidence.

Again, the statement in the Habakkuk Pesher that the wealth of the priests of Jerusalem would in the end be given to the Kittim[4] would be more fully relevant here than on any other proposed view. For we are told that Antiochus Epiphanes entered the Temple with the High Priest Menelaus as his guide, and that he seized the sacred vessels and votive offerings, and departed with no less than 1,800 talents.[5] Nothing comparable with this can be cited for the time of Alexander Jannæus, and in complete contrast with this we are told that when Pompey entered the Temple he removed none of its treasures.[6] It is true that ten years after Pompey's entry into the Temple, Crassus seized 2,000 talents from the Temple treasures.[7] It is unlikely, however, that this is what was in the writer's mind, since no one writing after this time could have supposed that Jannæus and his contemporaries were

[1] Cf. Josephus, *Antiq.*, XII, v. 4 (xii. 256), E. Tr. by R. Marcus in the Loeb *Josephus*, vii, 1943, pp. 131, 133. Cf. also *Apocalypse of Moses*, viii. 1, where with clear reference to Antiochus Epiphanes, it says " he shall crucify those who confess to their circumcision ".

[2] Cf. 1 Macc. vii. 26 ff.

[3] Cf. *J.B.L.*, loc. cit., p. 92. He supposes, indeed, that the departure from Demetrius of some of those who had invoked his aid was due to his unexpected decision to march on the capital. This seems unlikely, and it might be supposed that those who invoked the aid might have reckoned with the likelihood that if the intervention were successful Demetrius would come to Jerusalem.

[4] Col. IX, line 6.

[5] 2 Macc. v. 15 f., 21; cf. 1 Macc. i. 20 ff., and Josephus, *Contra Ap.*, ii. 7 (83 ff.), and *Antiq.*, XII, v. 4 (xii. 249 ff.).

[6] Cf. Josephus, *Antiq.*, XIV, iv. 4 (xiv. 72), *B. J.*, I, vii. 6 (i. 152 f.). Cf. also Cicero, *Pro Flacco*, xxviii, 67: *at Cn. Pompeius captis Hierosolymis victor ex illo fano nihil attigit.*

[7] Cf. Josephus, *Antiq.*, XIV, vii. 1 (xiv. 105 ff.), *B.J.*, I, viii. 8 (i. 179).

the last priests of Jerusalem. Nor do we have any evidence to suggest that Crassus stayed to persecute any group of Jews who were opposed to the Jerusalem priests on religious grounds.

I find, therefore, that the latest evidence not only brings new support to the view that the Kittim of the Habakkuk Pesher were the Seleucids, but supplies further evidence for the view that the Teacher of Righteousness lived in the time of Antiochus Epiphanes. If the view that I accepted in 1952 is correct, and the Teacher of Righteousness was the Onias who died in 171 B.C.,[1] this would supply a sufficient reason for his non-mention in the Nahum Pesher, which speaks of the persecution which took place after his death, when the crucifixions under Antiochus took place.

[1] Cf. *The Zadokite Fragments and the Dead Sea Scrolls*, pp. 67 ff.; also *B.J.R.L.*, xxxv, 1952-53, p. 140.

THE PRE-POTTERY NEOLITHIC BURIALS, JERICHO

By I. W. Cornwall

Introduction

In the season of 1952, one result of the excavations at Jericho was the discovery of the stone-built wall of a pre-pottery Neolithic town.[1]

This was sufficiently remarkable, for though Prof. Garstang's excavations between the wars had exposed pre-pottery Neolithic houses, it had not until now been suspected that they formed part of a defended town. In the last few days of the 1953 season, however, there came to light a cache of seven human skulls, of which the living features had been restored in painted plaster, the eyes being represented by shells. These unique objects have been interpreted by Dr. Kenyon[2] as the portrait-effigies of revered ancestors, evidently objects of veneration, or even worship, to their owners.

In the following season, 1954, excavation was continued beneath the floors of some pre-pottery Neolithic houses in Trench 1, and it was not long before human remains in some profusion began to appear. From the number of the bodies, the unusual positions in which some of the skeletons lay, as well as from the fragmentary and disturbed condition of many, it was evident at the outset that this was no common cemetery. Moreover, as work proceeded, similar burials, both isolated and in groups, were found in other areas (D.1 and E.1), below the floors of the latest pre-pottery Neolithic houses.

It became clear that here was an unusually large sample of a very early settled population, including individuals of all ages and of both sexes, and all within very narrow limits contemporary in their untimely end. The study of such a representative group was likely to throw light, not only on the strictly archaeological problems of their period and place, but also, if sufficiently complete material could be recovered, would perhaps enable the physical anthropologists to determine their racial type and affinities and thence, possibly, suggest whence the earliest known inhabitants of Jericho had sprung.

The condition of the bones left something to be desired. Not only were they exceedingly friable, but some, especially the skulls, had suffered considerable crushing and flattening deformation from the increasing weight of at least some dozen metres of overlying deposits, slowly accumulated as city after city rose and decayed in the ensuing ages on the mounting débris of its predecessors. It was found that, even using up-to-date methods of preservation and strengthening before attempting to move them, little hope

[1] Kenyon, *P.E.Q.*, 1952, 73.

[2] *P.E.Q.*, 1953, 87.

could be entertained of preserving entire even the main long bones of each individual for study in the laboratory.

If any at all adequate observations were to be made, it was evidently necessary for the material to be seen, studied and, if possible, measured *in situ*. Only after this could the attempt be made to lift and transport to England as much as could be saved in a condition fit for further measurements and observations to be made at leisure.

Not only so, but the presence of so many skeletons which could not yet be removed was seriously impeding the progress of the main work of the expedition, which was to carry the excavation in those areas down to the buried natural surface of the plain and so uncover the evidences of the earliest human occupation on the site.

In these circumstances, the writer was summoned in all haste from London. Travelling by air at a few days' notice, with the permission of the Director of the Institute and of Professor Zeuner, and with the generous financial support of the British Academy, through the good offices of Professor Wheeler, a fortnight was spent at Jericho in assisting to excavate, studying, describing and measuring the remains on the spot.

In this work he was untiringly supported by every member of the excavation staff who could be spared, even temporarily, from other tasks. Especial thanks are due to Miss Cecil Western for strengthening the bones *in situ*, lifting and repair, to Miss Nancy Lord for photography, Lady Wheeler for her admirably accurate drawings and to Miss Dorothy Marshall and Miss Una Fraser for their skill in uncovering and cleaning the skeletons. All these and others bore their shares in the tedious task of freeing the bones from the earth and cleaning them without damage, in preparation for measurements and recording. The first three, between them, took over, in addition, responsibility for the measuring and describing when, at the end of the fortnight, the writer was obliged to return to England before it could be completed.

General Situation of the Burials

Most of the burials exposed to date seem to lie below the intact plaster floors of the latest series of pre-pottery Neolithic houses: that to which the plastered portrait-skulls evidently belonged. In no case was any disturbance of these floors observed, which might have indicated the burial of the bodies from the level of these houses, though in Squares B.4 and E.2 graves were cut into walls and floors (Plates 1 and 3). These bodies, perhaps, do not belong with the majority of those here described. It is clear that nearly all antedate the laying of the floors. At the same time, only a very short interval can have elapsed between the deaths of the buried individuals and the construction of the overlying houses, for the bodies were disturbed, presumably by their successors, and the nature of the disturbance makes it clear that in many cases the bones were still united by their ligaments when last moved (see below).

Though they appear to have been originally interred in a more or less orderly manner, in few cases was there any indication in the sections of graves or pits having been dug for their reception. The earth surrounding the burials was the usual pale greyish-brown clay-silty material, full of fragments of mud-brick and plaster, of which the tell is chiefly composed, and which is certainly the product of the disintegration of the materials of earlier structures.

It seems, then, that the bodies were simply buried in the rubble and débris of their own houses, during a levelling of the site immediately following their deaths, presumably in preparation for the erection over them of the latest pre-pottery Neolithic houses.

Excavation and Measurement of the Bones

When first seen by the writer, several areas in which burials occurred had been painstakingly cleared already and the bones exposed and cleaned for photography. A further large number were brought to light both during and after his visit. In view of their crushed and friable condition, this was a matter of no small difficulty. The surrounding earth was extremely compact, and even sticky when moist, while the bones were weak and crumbly, the shafts reasonably firm, though soft, but the articular ends extremely delicate. A single misjudged touch from a tool was sufficient to break the thin outer table of compact bone, when the underlying cancellous tissue crumbled into a mass of tiny fragments, which, from their orange-yellow iron-stained colour when moist, closely resembled a well-known breakfast cereal. To free such tender material from the tenacious matrix with the articular surfaces sufficiently complete for the taking of measurements required skill and patience of a high order. It was in any case advisable to let the bones dry somewhat before proceeding. Drying was found to improve their hardness but when dry they became exceedingly brittle. Owing to the prevailing damp weather conditions, it was difficult to get them dry *in situ*. During showers and at night the exposed burials were covered with sheets of brown paper and tarpaulins. Once clear of surrounding earth, but before any attempt to lift them was made, they were treated with an aqueous emulsion of polyvinyl acetate dissolved in toluene (Vinamul) gently applied with a soft brush until the bone would absorb no more. This impregnation toughened them considerably when the solvent had evaporated. When sufficiently dry, some of the stouter bones were carefully lifted, after their position had been recorded. Even so, some were broken in lifting and had to be preserved in several fragments.

In view of the difficulties to be overcome and the consequent extreme slowness and uncertainty of the lifting process, it was thought advisable to obtain measurements *in situ* of such of the main long bones as had survived the process of cleaning sufficiently well. In its essence, this idea was simple enough. When the two ends had been sufficiently exposed it was proposed to take overall length-measurements with large callipers. Difficulties were at once encountered. It was hard to obtain enough clearance for the callipers

in the case of the more complicated groups of bones. Further, when, for example, a femur and tibia were still in their positions of articulation, it was necessary to plan a course of action involving the smallest loss. It was clearly impossible to measure one until the other had either been successfully lifted or sacrificed to permit the measurement of the first. Often, therefore, we had to clear both before it was possible to decide which was the more likely to provide a fairly good measurement. When, as was fairly often, the bones formed part of a much disturbed mass, this planning ahead assumed the complexity of a game of " fan patience ", or perhaps, more exactly, one of " spillikins ". Finally, when a bone had been successfully prepared for a measurement, it was difficult to be sure that the true maximum length had been obtained. Even the gentle touch of the callipers was liable to inflict some damage and there was no question of moving the rounded ends about, in contact with both surfaces, until the maximum gape had been assured. In the case of tibiæ the intercondylar crest was ignored and the measurement taken to the highest point of the medial condylar facet.

Under these circumstances, it is not surprising that the number of measurements which it was possible to recover was not large as as had been hoped, in view of the number of individuals represented. In the case of those long bones which were successfully lifted[1] and brought to London, it has been possible to check some of the measurements taken in the field. Most were found to agree very closely with those later made at greater leisure.

Description of Burials by Areas

Square B.4

No. 1 was placed in, or under the plaster floor of the temple (JPB4.45) in a shallow oval grave. (Plate 1A.)

The burial consisted of the much-crushed skull with the mandible and the upper part of the trunk, lying on the left side, head to the north. The bones were much disturbed: the ribs in disarray, the sternum and right clavicle facing upwards (to the body's right), the extended right arm, and the remains of the hand articulated as a limb, but the head of the humerus displaced and lying close to the right angle of the mandible. There were no scapulæ, no left arm and nothing preserved below the middle thoracic vertebrae. The radius was measured and lifted.

A worked flint point about 6 cm. long was found among the ribs, but there is no evidence to suggest whether or not this was a weapon which might have brought about the death of the individual. It might just as well have been a grave-offering. The skull suggested a young woman of about 20 years.

Nos. 2 and 3 were isolated skulls with mandibles, in a squarish, shallow hole cut through the same plaster floor as the above (JPB4.45) (Plate 1B).

[1] These were given Arabic serial numbers for identification, in the order in which they were recovered, since their assignment to a particular individual was often in doubt.

Both were much crushed and very fragmentary, but, from the general lightness of their build, appear to be youthful or of female sex. The one had an unerupted upper third molar, a fact which supports the former conclusion.

Nos. 4 *and* 5 (JBP4.46, below floor G.2.26) were another pair of isolated skulls, the one (No. 4) face upwards, chin to the north-west, with most of the face missing. The other (No. 5) had its vertex to the east and lay almost on its left side facing south. These two were actually in contact and had apparently been ceremoniously interred below the floor without special orientation. They were not seen *in situ* by the writer, having been discovered after his departure.

No. 6, in the same layer and below the same floor, consisted of a group of fragmentary limb-bones—a left forearm with traces of the hand, a fragmentary left femur and tibia, flexed in articulation. Lying obliquely across these was a right tibia with some foot-bones. The centre of this group lay some 70 cm. to the north-east of the former of the two skulls just described. It apparently represents a much-disturbed burial in a crouched attitude on the left side, the head having perhaps been to the east. While approximately contemporary with Nos. 4 and 5, there is no evidence to connect this body with either of them.

*Square D.*1

No. 1 (JPD10.8B) comprised remains of two individuals, designated A and B. (Plate 2A.)

A was the complete right half of a contracted burial on the right side, head to the east, with the skull in position. The left side of the burial had apparently been shaved off in antiquity during some levelling operation. The skull was evidently intentionally spared by the authors of this disturbance, for the foundations of Wall V had been built round it.

The individual was probably a male, judging by the absence of a pre-auricular sulcus and the general form of the right ilium and, on the evidence of much-worn teeth, elderly. The bones were too much crushed and incomplete as to their articular ends for any useful measurements to be taken. The fragmentary left humerus of a small ruminant (? sheep) lay below the right foot. The right femur appears to have been broken above the knee, probably *post mortem*, and the sternum had fallen, uninjured, into the thoracic cavity. The manubrium was apparently ankylosed to the body of the sternum —a further indication of maturity of age.

Body B of this group was represented by a pair of contracted legs, immediately to the west of A and only a few centimetres distant, belonging to a body lying on the left side, of which the trunk appears to have been below the pelvis of A, with head to the south. The rest of this, if preserved, was never exposed. The bones, though not as badly crushed as those of body A, lacked the necessary points for recording any overall measurement.

No. 2 (Plate 2B) was a headless contracted burial on the left side, head to

the east, elbows between the knees, hands near the face. Apart from the displacement of the skull (perhaps that, Serial No. 11, found facing in the same direction at a point 10 cms. in front of the knees, with mandible Serial No. 5 and an associated maxilla fragment) the body was more or less complete, though the upper ribs, cervical vertebræ and scapulæ had been disturbed. Another mandible (Serial No. 6) was found a few cms. west of the pelvis. There was no evidence as to the proper owner of this. From the evidence of the pelvis, No. 2 was a male. Some measurements of his bones were successfully taken.

No. 3 (6.c.1) included remains of two individuals. A consisted of a pair of tibiæ and some other bones, suggesting a disturbed and fragmentary burial, contracted, on the right side, head approximately to the north. The left forearm lay between the shins, the right forearm below the right knee, the hands clasped. Some fragmentary tarsals and metatarsals of the right foot lay in their correct positions. By the left shin, the conjoined ulna and radius of a (?) sheep. The rest of the body had been cut away in antiquity during some building operation. There was no indication of the sex.

B was the remains of an infant, only a few months old, consisting of fragments of the skull, vertebræ and the larger ribs, scapulæ, pelvis and lower limb-bones. The attitude was like that of an adult—contracted, on the right side, head to the north-east. The proximity of this body to that of A. suggested the possibility of their being related—mother and child—but A lay with its back to the child and so was, perhaps not, after all, to be associated with it.

No. 4 of this group was represented only by the distal part of a right femur and the associated broken tibia and fibula in the position of articulation. There were also some remains of the right foot. This body seems to have been in the same attitude as No. 5 (below), parallel with it, the knee and foot in actual contact with it when buried. A disturbance which only slightly affected the latter seems to have involved all the rest of No. 4. One loose mandible (Serial No. 3) perhaps belonging to No. 5, lay, inverted, upon the knee of No. 4. Another (Serial No. 7), associated with a single cervical vertebra, lay in the space probably once occupied by the trunk of No. 4.

No. 5 (JPD10) was more complete than the foregoing, a contracted burial on the left side, head to the east, only the skull (Serial No. 12) being displaced, lying upright, facing approximately north, close to the mandible (Serial No. 3) referred to above. From their proximity it would appear that this skull and mandible belonged to No. 5, but such was the confusion in many of these burials that mere proximity is no evidence of ownership. Judging by the pelvis, this body was that of a female. As in others, the arms were apparently folded on the chest, the elbows between the knees, but the left side was not exposed when seen. A fragment of the left horizontal ramus of a mandible with two teeth was in position about where the skull of No. 5 should have been. It thus seems more likely to be the mandible of No. 5 than that lying near the knees of No. 4.

The group in Square D.1 thus consisted of two more or less complete bodies (Nos. 2 and 5) two skulls (Serial Nos. 11 and 12), seven mandibles or fragments and some loose teeth and the right knee, which is all that certainly remains of No. 4.

Square E. II

No. 1 (Plates 3A and B). This was a very closely-contracted burial on the left side, head to the south, buried in a hole cut into a mud-brick wall. The lower part of the trunk and the pelvis were missing, but the skull and mandible were in position. The right knee was drawn up and covered the right cheek, the right arm being flexed and passed under this knee, between the thigh and the leg, so that the middle of the shaft of the right femur lay below the elbow. The right hand rested on the vertex of the skull. The bones of the left side were not clearly exposed when seen, but the left forearm was seen to lie below the right hand. In the absence of the pelvis, the skull was examined for evidence of sex. The right mastoid process was missing but the nuchal muscular impressions suggested a male. The frontal region was insufficiently well exposed for any evidence in support of this suggestion to be seen at that time. None of the bones could be measured, being too incomplete.

No. 3 (JPE11.11). This individual consisted only of the upper part of a trunk, lying half-prone, head to the south-east. The cranium was missing but the mandible was present in the prone position, presumably undisturbed. The right arm was flexed, the right hand flat to the ground beside the head, but the right humerus was missing and the scapula was found close to the right elbow (Plate 3a, left). The left arm, forearm and hand were folded under the body and head. A fragment of the shaft of the right humerus lay somewhat to the south of the body.

The teeth of the strong mandible were worn, suggesting an elderly individual, the left second molar lost during life. As seen *in situ* there was no evidence of sex save for the general build, which suggested a male. No measurements of individual bones were possible.

*Square F.*1

Nos. 1 *and* 2 (F4.23, north of Wall N). (Plate 4A.) This group first emerged as a pair of skulls, closely side by side in a vertical posture, facing the north. No. 1 was to the east, No. 2 to the west. Behind them, to the south, there next appeared the upper part of a body, prone, the right scapula and humerus in position, the left apparently missing. The lower part of this body was not seen, being buried, if still preserved, in the balk separating this Square from Square D.1 from the lumbar region downwards. It is hard to say to which of the skulls (if to either !) the body belonged.

To the north-east of skull No. 1 there was later revealed the left humerus and scapula, correctly articulated, which probably belonged to the body,

since there these parts were missing. At right-angles to the shaft of the humerus and lying under it, was a radius, probably part of the same left arm, but disarticulated. Of the ulna there was no trace. The proximal end of the radius, pointing about north-east, lay across the molars of a disarticulated mandible (1), while another loose mandible (2) was found 30 cm. to the south, near the shoulder-joint of the displaced humerus and scapula. Underneath these emerged later a sacrum and some loose ribs, in no particular order. Close to the distal end of the humerus lay three lumbar vertebræ.

Apart from one intrusive skull and mandible, therefore, the whole of this group, as far as exposed, could have belonged to the one body. An interesting feature was the displaced, but still articulated, left scapula and humerus. For the bones to have maintained their correct anatomical position the joint must still have been held together by at least its ligaments when the bones were removed from their proper positions in relation to the rest of the body. The other parts must have been shifted at the same time and not only the proper remains of the one individual, but also the skull and mandible of another have been returned with some care (at least as regards the skulls) to the hole which must have been dug to expose the head end of the body. As the skulls were not in this instance taken, the disturbance seems rather to have been the work of tomb-robbers than of skull-seekers.

The whole group was lifted in a block and is now in Amman Museum, so that the remains have not been available to the writer for study since they were first seen *in situ*.

An interesting feature was the presence of a still-open metopic suture in skull No. 1. Both this and its companion appear to have belonged to individuals aged not more than 25 years, as the sutures of the vault were all clearly traceable in both (see photo). They had been much crushed and distorted by earth-pressure from above since their final interment. An unbroken plaster floor of a pre-pottery Neolithic house sealed the level in which the remains were buried, its upper surface being no more than 10 cm. above the bones (see photo Plate 4A).

Nos. 3 *and* 4. A few metres to the east of Nos. 1 and 2, and at a slightly lower level, there began to appear an apparently inextricably tangled mass of human remains, extending over an area approximately 2 m. by 1 m. (Plate 4B). This largely consisted of legs and pelves, all more or less disarticulated. Two bodies only stood out reasonably clearly from the rest and these were labelled Nos. 3 and 4.

Neither had any skull and indeed both were considerably disturbed, but what remained in position showed that there had originally been a pair of flexed burials on the left side, heads to the south.

No. 3, the more easterly of these, was an adolescent, perhaps 12 to 14 years of age, judging from the stature. The thighs were only half flexed on the trunk, but the knees were sharply bent, so that the ankles were close to the pelvis. The arm bones, broken and somewhat displaced, suggested

folded arms, the hands near the face. The body of the sternum was entire but had fallen into a position near the right elbow. A fragment of the right scapula, half a dozen of the larger ribs, some lumber vertebræ and the pelvis all more or less in position, completed this body.

No. 4, rather more disturbed, seemed to be the twin of No. 3. The pelves were only about 30 cm. apart, the trunks and limbs in almost exactly the same attitudes. The trunk of No. 4 was indicated only by some ribs, still in position with the fragmentary arm-bones. A gap between the middle ribs and the pelvis was occupied, at a lower level, by an adult mandible, a fragmentary scapula and some other broken bones. The right leg was almost complete, the knee over the ankle of No. 3, patella in position.

As for the rest of the large group, little order at all could be seen at this stage. There was, in the middle, a fairly complete adult right leg, articulated, complete from the os coxæ to the ankle, with the tightly-flexed knee pointing roughly to the north-east. Other long bones, mostly of thighs and legs, were in complete disorder.

In view of this, it was hoped that an approximation to the histological technique of " serial sections " might assist in interpreting the tangle. If each layer of bones exposed were drawn to scale on tracing-paper and the drawings super-imposed, the exact relations of the remains found in each layer to those underneath would be shown and this would perhaps make it possible to assign bones to their proper owners.

After layer (*a*), the first exposed, had been thus recorded, a drawing of (*b*), underneath it, was prepared as excavation proceeded and in the case of the present group a third layer, (*c*), was also found necessary. Layer (*b*) revealed the complete left leg, from the knee down, and the foot of the owner of the leg referred to above (called " A "), which was immediately below the right, exposed in the previous layer. Assuming this to be a flexed body on its left side, the head approximately to the north, a scapula with articulated broken humerus in layer (*a*), some ribs, another scapula and the bones of a forearm in layer (*b*) fell into place as probably belonging to it. Perhaps the displaced mandible (Serial No. 4) also belonged to body A.

Search for other remains attributable to A led to a group of bones perhaps 30 cm. to the north-west of the right *os innominatum* in layer (*a*). This consisted of another innominate bone (Serial No. 10), a broken femur and some loose ribs quite out of position but, from their large size, possibly belonging to A. A tibia underneath the *os innominatum* was perhaps the fellow of that lying parallel to it in layer (*b*), since we already have a complete leg for A. Just to the north of the scapulæ of A was a pair of small scapulæ, in layers (*a*) and (*b*) respectively, both with their dorsal sides downwards, suggesting the original presence of another body (B), supine, of which all but these bones have been destroyed or scattered. A small mandible (Serial No. 13) in layer (*b*) may also have belonged to this body, as also a forearm, represented by a very slender ulna and radius in layer (*b*), lying among the ribs of A.

1A. Square B 4. Burial No. 1, in the plaster floor of the Temple. View from S.W. Note the flint point among the ribs immediately below the scale.

1B. Square B.4, Burials 2 and 3, cut into the plaster floor of the Temple.

2A. Square D.1, Burial 1. View from N.

2B. Square D.1, Burials 2(A) and (B), from S. The two disturbed skulls cannot yet be assigned to either body.

3A. Square E.2. General view of the burials from E. No. 1 in the right background, Nos. 2 and 3 in the right and left foreground respectively.

3B. Square E.2. Detail of Burial No. 1, the grave cut into a wall.

4A. Square F.1. Skulls 1 and 2 and the single body associated with them. N. of Wall N. View from N. Note above the body the intact plaster floor of a house belonging to the period of the Plastered skulls found in 1953.

4B. Square F.1. The group including burials 3 and 4, seen from W. The twin youths (No. 3 to the right) are at the top right of the photograph.

5A. Square F.1, Burial No. 5, from S.

5B. Square F.1. The group called Burials 6 (A) and (B), from S. No. 6(A) is the single individual on the left. No. 6(B) proved to consist of remains of at least eight individuals. The photograph shows only the uppermost layer exposed. Six horizontal "serial sections"

6A. Square F.1, Burial No. 10, from the N.E.

6B. A pair of skulls in Square F.1.
(All photographs by N. Lord, 1954)

The rest of the bones in layer (*a*) at the south-west corner of the area, were so obviously disturbed that they gave no indication of the attitude of any body. At least two are represented. Even taken together with those below them in layers (*b*) and (*c*), little order was apparent in the bones. The left femur (Serial No. 1) in layer (*a*) looks like the fellow of that immediately beneath it, but they are mutually reversed in position. The pair of tibiæ in layer (*a*), which possibly belonged to the same individual, lack the feet and have no sensible relation to what lies underneath, in layer (*b*). Here, a pair of scapulæ, loose ribs, vertebræ and a clavicle lie in disorder with two more disjointed tibiæ, one with its fibula. Only one thing emerges clearly—that amid all the disorder whole segments of limbs had been disturbed as units, the tibiæ and fibulæ together, the ulnæ and radii still in correct articulation. There are, in layer (*b*), for example, three cases of the latter.

The disturbance evidently involved the whole group more or less, for, though in layer (*c*), at its eastern edge, there is an entire right arm and a pair of legs, complete save for the right femur, nothing remains of the rest of the body, nor does any missing part appear to lie in the layer above. The remains seem to represent, more or less, a contracted burial on the right side with the head to the south.

In the middle of the area, in layer (*c*), lies a pair of humeri, side by side but reversed. Another pair of humeri, perhaps once connected with the disjointed forearms in the southern part of layer (*b*), are associated, in their own layer, only with a disjointed pair of innominate bones and two loose ribs.

Now all this end of the area, in layers (*b*) and (*c*), lies underneath the two still fairly recognizable youthful burials, Nos. 3 and 4. It appears, therefore, that the burial of these, at least, must be subsequent to the major disturbances in the deeper layers. At the other end of the area there is nothing to show that the disturbance did not all take place on a single occasion, for all three layers are involved, but it seems as if the more eastern part of the area was immune, for it is here that the only remains still in position were found.

In all the group there is not a single cranium, but there are no less than nine detached mandibles. These lie in all sorts of attitudes, as if discarded and thrown down among the rest of the disturbed remains. These facts perhaps give a clue to the purpose of the disturbances—a deliberate exhumation of the bodies to obtain the skulls. That this grisly search took place not longer after the original burials than a few weeks at most is shown by the many parts displaced and disordered as regards their relations to other parts of the same skeleton, and yet still correctly articulated with their neares. neighbours—tibiæ with fibulæ, ulnæ with radii, and scapulæ with humerit Nevertheless decomposition of the bodies must have been fairly well advanced, otherwise how to account for separated innominate bones, femora and humeri separated from their respective distal segments, reversed in relation to their fellows, and the numerous disarticulated mandibles ?

One further possibility suggests itself—that the bodies were exposed, deliberately or by neglect, and only interred when decomposition was so far advanced that whole limbs separated readily from the trunks. It scarcely seems likely, however, that limb bones, or even two bones in the same segment of a limb, would remain articulated if decay of the body had proceeded so far as to permit separation of the parts of the pelvis or of mandibles from the skulls. There is, further, the matter of the fairly orderly contracted burials. These show the inhumation of at least some complete bodies and the fact that an originally orderly burial is discernible in many badly-disturbed cases suggests that the majority, at any rate, were not dismembered when first placed in the ground. The absence in so many cases of the skulls suggests a reason for the subsequent disturbances—a search among the remains for trophies or mementoes of the deceased.

*Square F.*1, *No.* 5 (*F*8.9*c*) (Plate 5A).

This was a complete skeleton with skull in position in the extreme north-east corner of Square F.1, the head in the wall of the trench. The body lay on the left side, flexed, not contracted, head to the north, the arms extended, the left between the legs with its hand near the feet. The remains were those of a young male (sexed by the pelvis) with incompletely fused epiphyses of the long bones, 17 to 18 years of age. Only the hinder part of the right side of the skull could be seen, so that no confirmation of the age-estimate could at that time be obtained from the teeth. The vertebral border of the right scapula was incompletely ossified. Despite the poor condition of the epiphyses, some of the bones were measurable. A large slab of stone stood on edge behind the body, but whether this was fortuitous or formed any intentional part of the burial did not appear.

*Square F.*1, *No.* 6. (Plate 5B.)

As first seen, burial No. 6 was in two clear groups, separated by a distance of a few centimetres: that more to the south-east clearly consisting of only a single individual, designated 6A. The whole occupied an area rather more than 1 m. square.

As with Nos. 3 and 4 above, it was found necessary to record the positions of bones in several layers, since 6B evidently comprised several individuals and extended to an unknown depth.

No. 6A was a tightly-contracted burial, head to the south, lying on its back so that the knees and shins were uppermost and appeared first in the course of excavation. This curious posture suggested that the body had been first bound to keep it compact and then buried rather unceremoniously in the smallest possible hole.

The skull was missing, as was the pelvis, save for a fragment of the sacrum which appeared later, but the mandible (Serial No. 7) was found near the left knee, having evidently been discarded by skull-robbers. Otherwise the

body was complete, though not undisturbed. The left leg and foot were in a position compatible with the cramped posture, considering that the pelvis had been disturbed and the head of the femur in consequence perhaps somewhat displaced. The right femur was likewise approximately in position, though its head had been moved. Under the head of this bone, however, was found the *proximal* end of the corresponding tibia and fibula with the patella still in position, while the articulated foot was close to the place where the face would have been had the skull still been there. Not only, therefore, had the skull been removed and some disturbance of the pelvis also taken place, but the whole right leg had been lifted from the grave in two pieces (the thigh and the leg with the foot) and apparently carefully (though incorrectly) replaced. For the patella and foot to be still articulated with the tibia and fibula the leg must have been in one piece when moved, the ligaments still holding the bones in position.

The presence of the rest of the body was indicated by the five lumbar vertebræ in their correct positions sloping down into the ground cranially. When the legs had been removed (the two femora, Serial Nos. 5 and 6, were safely lifted, with the mandible, Serial No. 7), the upper part of the trunk and the arms and hands were found. The right humerus, in articulation with its scapula, lay across the chest, the forearm flexed and the hand near the left shoulder. The right clavicle lay directly underneath the upper part of its shaft. The left humerus and scapula were also present, the former somewhat abducted to the left, but the forearm and hand on this side were missing and did not appear at any lower level, having apparently been involved in the disturbance which had affected the adjacent group of bones to the west. The last four cervical vertebræ were also present. The ribs and sternum had fallen into the body cavity[1] in contact with the vertebræ but showed no unusual displacement. Save for the cranium and first three cervicals, the pelvis and the left forearm and hand, this individual was, therefore, complete, as originally buried but the right leg and foot had been disarticulated at the knee and turned end-for-end before their replacement after disturbance of the burial.

*Square F.*1 6*B*. (Plate 5B, right.)

The group designated as "6B" revealed itself, on being cleared for photography, as an almost inextricable confusion of disturbed bones extending to some depth. On closer inspection, a few of them were seen to be still correctly articulated in small groups—half a dozen vertebræ here; a humerus, ulna and radius there; two conjoined tibiæ and fibulæ; a humerus still with its scapula. On the whole, however, no natural order could be seen and it

[1] This, once more, shows that the thorax must still have been covered with soft tissues at the time of interment—otherwise earth would have sifted into the thoracic cavity between the ribs and have supported the sternum when it fell loose.

was obvious from the relative sizes of the bones that at least three or four individuals were concerned.

The disturbance, presumably in search of crania—for none (or, rather, only one occipital fragment with the condyles) was found in all this mass—had apparently ended with the more or less orderly replacement of several limb-bones near the top and towards the south-east side of the group. There were, for example, in the uppermost layer four femora (Serial Nos. 8, 11, 12 and 16) in a group, lying with their shafts almost parallel, three of them with their heads to the north-east, one reversed. One much smaller, youthful, left femur lay close to its tibia and fibula, in the attitude of a contracted leg, near the middle of the group. Four humeri, of a size comparable with the femora, were also visible in this layer, one of them, at the north edge of the group, still correctly articulated with its forearm bones. Another, a right humerus, lay at the opposite side in articulation with a scapula, associated with some ribs more or less in position, an atlas vertebra and the small fragment of an occipital bone. An ulna and radius, separate but parallel, near the east corner of the area, were of a size to consort with the youthful leg bones mentioned above, as were also two groups of vertebræ—seven thoracics with some ribs at the north end, four cervicals, a clavicle and a mandible (Serial No. 1) near the knee of the disarticulated leg.

At least three individuals are, therefore, represented: two adults (i) and (iii), owners of the larger long bones, and a youngster, (ii).

Layer (*b*) showed three more humeri, two of differing adult sizes, in position with their respective right scapulæ, (*x* and *y*), and the right humerus of a child, but one too small to have owned the youthful bones already mentioned. This indicates two individuals, a smaller (probably identical with (iii) above) and a larger adult (iv), with the child (v.) A very small scapula, evidently also of (v), lay near the middle of the west boundary of the area.

In layer (*c*) appeared a complete contracted left leg and foot (femur Serial No. 15, tibia Serial No. 14). The entire left arm and hand of the child (v) was found almost below the right humerus in the previous layer. A fragmentary left adult ulna and radius and some carpal bones seemed to belong to the individual (iii) owning the adult leg. These bones were joined with the humerus and scapula of (iv) in the previous layer and so the leg and foot probably belong to (iv) also. A complete adult left foot, separated from its leg bones (not identified) occupied the north-east corner in this layer.

Layer (*d*) was not so confused as those hitherto described. In the middle the femur (Serial No. 15) of (iv) was found to be in articulation with its *os innominatum* (Serial No. 24). Across the ilium lay the shaft of a very long left femur (Serial No. 16), in articulation with its tibia (Serial No. 19) while a large left tibia, fibula and articulated foot lay directly beneath one of the femora (Serial No. 11) of layer (*a*), but not, apparently, in articulation with

any of the femora of that layer. In the north-east corner of the area were found two loose mandibles (Serial Nos. 20 and 21), an *os innominatum* (Serial No. 25) and a number of thoracic and lumbar vertebræ, with some disarranged ribs. Six of the thoracics formed a group still in position, suggesting a body lying on the left side with head to the south-east. This may, perhaps be the owner of the *os innominatum* (Serial No. 24) and its articulated right femur (Serial No. 15).

To the north-west of these were two clear flexed burials, heads to the north-east, lying on the left side, headless, but with the respective mandibles thrown back (Serial Nos. 17 and 18). The former consisted of the trunk, arms and mandible only, and was adult. The other was of a child.

Layer (*e*) consisted only of an *os innominatum* (Serial No. 23) some ribs and a vertebra.

Under these again, in layer (*f*), was the trunk of another youthful individual.

Burial group 6B seems to tell a story similar to that deduced from Burials 3 and 4. Skull robbery was the apparent motive for rummaging the group to its very bottom layer. This remained relatively undisturbed once the object of the search had been achieved, while the bones higher up in the mass had clearly been first taken up bodily and then replaced in confusion. Some lingering respect for, or fear of, the dead had, perhaps, prompted the fairly orderly replacement of the group of disjointed long bones near the summit of the collection.

It would appear, then, that the bodies were first interred entire, if unceremoniously, one on top of the other. This possibly bespeaks a hasty evacuation of the city following a deadly plague—for not one of the bones bore signs of violence such as might have been expected as the result of a massacre. Shallow common graves were dug, the bodies heaped in without regard to age or sex, but with a certain care as to their attitudes. Where any natural arrangement at all could be observed, the bodies seem to have been laid, flexed or contracted, on their sides, not thrown in with limbs outspread as might have been the case with enemy casualties treated as unrespected carrion.

Later—after no long interval—came the skull-quarriers, intent only on their thorough search, for not one cranium in the whole mass escaped them. Legs and arms, whole trunks they dismembered, wrenching the mandibles from the skulls as they were found and throwing them down among the rest of the unwanted, rotting remains. Whether these ghouls were strangers, or the surviving kin of the deceased intent on ritual preservation of the most characteristic parts of their late relations cannot now be told for certain, but the existence of the plastered skulls in the next houses points to some such motive. Doubtless there are other caches of the numerous missing skulls somewhere about the tell.

This does not complete the enumeration of the burials discovered in Square F.1 in 1954, but most of the remainder were so fragmentary or

confused that their description must await the conclusion of the study, still in progress, of the remains recovered and brought to England.

Two only are here illustrated.

No. 10 (Plate 6A) situated in the middle of the Square, was a crouched burial on the left side, head to the south-west, reasonably complete save for the skull. This had apparently been taken by skull-seekers, but a mandible was found close by which seemed to have belonged to this body. The individual is considered to be a well-developed adult male.

Plate 6B shows a pair of skulls discovered at the same level as the rest to the north of wall N. Evidently, not all of the skulls missing from elsewhere were removed with the object of restoring them to their living appearance with plaster and paint. Some were separated from the rest of the body and carefully re-interred, often, as may be seen, in pairs (Plates 1B and 4A, and others not illustrated). The reason for this can now only be guessed. Possibly they were, in some cases, man and wife, or close friends, whose bodies, having been buried by chance apart, the skulls were later reunited by surviving friends or kin. Less obvious superstitious or ritual motives can be imagined, but speculations unsupported by any evidence are unprofitable, if not actually misleading. Perhaps the social anthropologists can point to similar practices among surviving primitive peoples, which might provide a plausible explanation.

THE EXCAVATIONS AT TELL EL-FAR'AH AND THE SITE OF ANCIENT TIRZAH

By R. DE VAUX, O.P.

Tell el-Fâr'ah, an ancient site in Palestine, lies to the north-east of Nablus, on a road which runs from Nablus to Beisân. The hill rises between two springs, at the beginning of a large valley, the Wadi Fâr'ah, which slopes down to the Jordan. The French Archaeological School in Jerusalem began excavations there in 1946. Three motives had led to the choice of this site.

First, an historical problem: Tell el-Fâr'ah lies at a point where natural highways meet to form a cross-roads, near to splendid springs and at the head of a fertile valley. It seems to combine all those conditions which favour human settlement. When was it first occupied ? How long has it lain abandoned ? And why ?

Secondly, there is an archaeological problem: the last fifty years have seen extensive excavation in both the north and south of Palestine. A series of civilizations has thus been revealed, all developing or replacing each other in essentially the same way, though there are marked differences from one region to another. Between these archaeological regions of north and south, the centre of Palestine was little known, apart from Samaria. Yet the history of Samaria reaches back only to the ninth century B.C.—which is a very late date for an archaeologist in the Near East. Tell el-Fâr'ah might throw some light on the past of this central region, and thus provide the link between the north and south.

Finally, there was a problem of biblical geography: this site, which must have played a part in ancient history, has been identified with various places mentioned in the Bible. Dalman,[1] followed by Alt,[2] suggested that it was Ophrah of the Abiezrites, the home of Gideon. Albright, however, proposed Tirzah, Jeroboam's capital,[3] while Fr Abel[4] suggested the Beth-barah mentioned in Judges vii. 24. Excavations might authorize us to decide between these hypotheses.

After six seasons of digging,[5] the interest of the site is not exhausted, and

[1] *Palästinajahrbuch*, VIII (1913) ,pp. 31 f.

[2] *Ibid.*, XXIII (1927), pp. 36 f.; XXVIII (1932), pp. 40 f.

[3] *Journal of the Palestine Oriental Society*, XI (1931), pp. 241 f.

[4] *Géographie de la Palestine*, II (1938), p. 268.

[5] Preliminary reports have appeared in the *Revue Biblique*, LIV (1947), pp. 394-443; 573-589; LV (1948), pp. 544-580; LVI (1949), pp. 102-138; LVIII (1951) pp. 393-430; 566-590; LIX (1952), pp. 551-583; LXII (1955), pp. 541-589. The report on the sixth season (1955) has not yet been published.

work will continue, but already it is possible to give a satisfactory answer to these three questions.

1°—Tell el-Fâr'ah was occupied from the beginning of the Chalcolithic Age, in the fourth millenium B.C., until roughly 600 B.C., with a break of several centuries between the middle of the Early Bronze and the beginning of the Middle Bronze. This long interruption and its ultimate abandonment are not to be explained by purely historical causes; the spot is unhealthy, with a great deal of malaria, and this has perhaps been a contributing factor.

2°—As we should expect, Tell el-Fâr'ah is most closely linked with the civilizations in the north of Palestine. But the excavations have brought to light interesting contacts with the south, especially in the very early periods, whose relative chronology is thus more securely established.

3°—As regards its identification, it now appears extremely probable that Tell el-Fâr'ah represents ancient Tirzah, the capital of the Northern Kingdom before the foundation of Samaria.

In presenting the results of the excavations, I shall choose that which illustrates these three points, and shall lay special stress upon the last, by reason of its strictly biblical interest.

The Chalcolithic Age.

Tell el-Fâr'ah is a hill rising between two springs, 'Aïn el-Fâr'ah to the north and 'Aïn ed-Dleib to the south, the waters of which quickly meet to form the Wadi Fâr'ah. On the north and south the hill rises steeply, it dominates the eastern approach, and hence needs to be fortified only on the western side, where a gentle slope joins it to the mountain. It commands Wadi Fâr'ah, which is the only wide pass leading from the Jordan valley into the heart of Palestine. Here Abraham and Jacob must have passed with their flocks on the way from Mesopotamia to Shechem. A valley, narrower and deeper, climbs from Tell el-Fâr'ah towards Nablus. Another road leads northwards towards Beisân. These routes have been followed from time immemorial; they formed the basis of Roman roads and have been adopted as the lines of communication in modern times. Tell el-Fâr'ah is, then, a strategic point, straddling a knot of communications: in ancient times it was a fortified town; the Arabs, probably under Saladin, built a watch tower nearby and a modern police post stands there also.

Yet, in spite of all these natural advantages, Tell el-Fâr'ah is to-day uninhabited. The three nearest villages are Talluza, Tubas and Tammun, all some three miles away from the springs where their territories converge and where they bring their flocks to water. The reason is probably to be sought in the malaria with which the site is infested. According to the records of the Health Service, in 1946 nine-tenths of its tiny resident population were infected with malaria and I myself caught it during the dig. Since then, steps have been taken to improve the situation, and since 1948 a large refugee

camp has grown up near the waters, agriculture is developing, and the area is once more regaining a little of the life it knew in ancient times.

We have opened two fields of excavation on the Tell: one to the north, the second, larger and more rewarding, to the south-west. It appears that during the Chalcolithic and Early Bronze Ages the entire hill was inhabited, but from the time of the Middle Bronze occupation was limited to the south-west region, where the highest point of the tell forms a natural acropolis.

Apart from a neolithic pit—which so far is an isolated specimen—the first evidence of human dwelling on any considerable scale belongs to the early Chalcolithic. Potsherds of this period are found above the virgin soil in every place where we have reached it, but so far no traces of buildings have been uncovered. The pottery is related to that of Beisân and Megiddo, of Teleilât Ghassul and of Jericho. Thus, from the very beginning, our interest in Tell el-Fâr'ah as a meeting-place between the civilizations of the north and south of Palestine has been justified.

In 1955 we found a new example of these contacts. In the escarpment on the south side of the Tell we found a cave which had been used for a fairly long time during the Chalcolithic Age. At the back is a bench with several shallow basins, cut in the rock, around which were fragments of pottery. Several skeletons were found, at two different levels. The pottery is not the same as that which is found in the lowest levels of the Tell, but its paste, workmanship and shape are identical with the kind which has recently been unearthed at Beersheba, one hundred miles further south.[6] Two groups, then, with different origins and different traditions, must have lived side by side at Fâr'ah, and this contact underlines what appears to have been a characteristic of the Chalcolithic Period in Palestine: different civilizations existed side by side, or blended with one another, across the entire land. This means that the country must have been but sparsely populated, and by people who were not firmly rooted to one region and who had no political organization.

The late Chalcolithic is far better represented, though its architecture is still uncertain. In our southern excavation, in spite of a layer rich in potsherds, no wall has been discovered from this period; in our northern dig, however, certain walls, sometimes of stone, sometimes of brick upon a stone foundation, *could* be dated so early, but not with complete certainty. Nevertheless, the potsherds of this time are plentiful, and, above all, the great chalcolithic tombs with their rich collection of vases—intact—show that this must have been one of the most prosperous periods in the history of the Tell.

We know of two—perhaps three—chalcolithic cemeteries. The most important lies south of the Tell, on the far side of the Wadi ed-Dleib. The tombs are either fairly big rooms hewn out of the rock, or natural caves

[6] J. Perrot, *The Excavations at Tell Abu Matar, near Beersheba*, in the *Israel Exploration Journal*, V (1955), pp. 17 ff.; 73 ff.; 167 ff.

smoothed out with flint tools, the marks of which we can still trace. They open into the side of the hill by a low door. It is not possible to determine the position of the skeletons, because the bones have been almost completely destroyed by natural agents, or else were scattered when the tombs were re-used in later times. All we know is that they were communal tombs, and that funeral offerings were numerous. These offerings consisted usually of pottery, and these tombs of Fâr'ah have yielded the most important and the most complete collection from the late Chalcolithic which has yet been found.

The pottery is of two kinds: a red, burnished pottery, which previously was almost unknown, and a grey, burnished type, which had been found at Megiddo, Beisân and Affuleh, and which was considered to be characteristic of the civilization in the plain of Esdraelon. The discoveries at Tell el-Fâr'ah show that it was known much further south, though it must have originated in the far north, in Asia Minor or in the eastern basin of the Mediterranean.

On the other hand, this pottery, which characterizes the late Chalcolithic in northern Palestine, is found at Tell el-Fâr'ah alongside pieces which would be classed as belonging to the Early Bronze in southern Palestine, where the late Chalcolithic is scarcely represented at all. One can therefore conclude that the lovely Chalcolithic pottery survived in the north while the civilization of the Early Bronze was growing up in the south—a civilization which was subsequently to spread over the entire country. This new civilization marks the arrival, about 3100 B.C., of a new wave of immigrants—the first to whom we can give a name—the Canaanites. Whatever their origin, they settled first in the south of Palestine, and this conclusion is a new light on the obscure beginnings of the history of the country.

Nearly all the tombs of the southern cemetery were re-used in the Middle or Late Bronze. But there is another cemetery which is solely Chalcolithic, lying just over half a mile north of the Tell: it seems that in the early days the inhabitants preferred to bury their dead at a distance from their homes. These tombs (Pl. XXIII, 1.), dug out of a very soft limestone, are of the same type as those in the southern cemetery and contain the same pottery.

Finally, one tomb has been found where the Tell juts out eastwards. So far, it is an isolated discovery, but it may perhaps point to the location of yet a third cemetery.

The Early Bronze Levels.

The town continued to flourish in the Early Bronze Age, during the third millenium B.C. But the evidence which archaeology offers is just the opposite of that from the Chalcolithic Age. Except for one chalcolithic tomb which was re-used in Early Bronze II, we do not know where the dead were buried, though we have six different levels of dwellings.

Three of these levels are earlier than the wall of the town (Pl. XXV). Houses are built on the sites of older dwellings, with only slight modifications of plan.

They are rectangular in shape, with stone walls, which sometimes support a superstructure in brick. We find slabs set according to the axis of the building or placed against the walls; they served as bases for the wooden pillars which supported the roofing. Lovely silos for corn were built with large stone slabs carefully smoothed over. The pottery of these three levels is characteristic of Early Bronze I and of the beginnings of Early Bronze II—that is, about 3100 to 2800 B.C.

Some of this pottery was made on the spot. We found a potter's oven (Pl. XXIV, 1.) at the level which immediately precedes the rampart. It comprised a lower section where the fuel was burnt and an upper part where the vases were placed to bake: the two sections were separated by a floor perforated by flues and supported by a pillar. Above the floor there must have been a brick vault with a chimney. This is the oldest potter's oven yet discovered in Palestine, and probably one of the earliest that existed in the country, for, as far as we can see, the closed oven was not used in Palestine before the Early Bronze. It is interesting to find in use from the very beginning a type of oven with flues which persisted for thousands of years, for we have another one, from Roman times, at Khirbet Qumrân.

About 2800 B.C. the town was surrounded by a wall for the first time. On the western side, it appears as an enormous line of bricks built over a foundation of stone. More than ten yards wide, it has survived in certain places to a height of thirteen feet. On the northern side, it is a stone structure over nine yards wide and six feet high, and protected by a glacis of trampled earth. It is built over the earlier levels of the Early Bronze. After it had been destroyed and abandoned, it served as a base for the wall of the Middle Bronze, which is much more narrow and which is protected on the outside by a glacis of small stones.

Two levels of buildings date from the same time as the first rampart; then the wall itself was torn down. The results are especially noticeable on the western line, where enormous piles of bricks have fallen inside the town. The date of this destruction is fixed about 2600 by the pottery.

The inhabitants quickly re-established themselves on the ruins: some houses were built above the bricks of the fallen rampart. They are of the same type as those we know already, rectangular, with some brick walls rising above a foundation of stones. Sometimes there is a low bench along the walls, sometimes a stone slab in the centre to hold the pillar which supported the roofing. This re-occupation, however, was apparently not very important, and did not last long, for the pottery comes to an end about 2500 at the latest. The six levels of the Early Bronze at Tell el Fâr'ah lasted less than six centuries.

The Middle Bronze Age.

The site was then abandoned, and remained so until well into the Middle Bronze. Other Palestinian sites also lay abandoned about the same time:

Megiddo, Beisân, Khirbet Kerak, Râs el-'Ayn, Aï. This retreat of sedentary civilization was caused by a new wave of Semitic immigrants—the semi-nomad Amorites. They are responsible for the decadence which marks the end of the Early Bronze, and the civilization of the Middle Bronze will begin to flourish only when they too settle down in towns, and under the pressure of new cultural influences from the north. But Tell el-Fâr'ah was abandoned earlier than almost any other site, and lay deserted for a longer period. In addition to the historical factor just mentioned, the unhealthiness of the spot perhaps contributed to this.

When the Tell was re-occupied in the 19th century B.C., habitation was restricted to the south-west part of the hill, which was the most elevated. A new rampart was built along the line of the old one. It was a stone wall of varying thickness, from six and a half yards on the northern side to two yards on the west. In the south-west corner it was reinforced by a bastion.

It was, in fact, only the inside line of defence, for it was protected on the northern side by a glacis of boulders and on the west by a large glacis of red earth held in position by a solid wall. Near the gate of the town, the most vulnerable spot, the combined fortification of rampart and glacis was sixteen yards thick. This wall, which lasted for a long time, is sufficient proof that Tell el-Fâr'ah was a royal city of the Canaanites.

The gate itself was well defended. It was preceded by an open-air approach, which had to be entered at right angles to the gate: thus the attackers would present their right flank, unshielded, to the defenders on the rampart. From this approach, one passes into a bay where the thresholds of the Middle Bronze, Late Bronze and Iron Age are built one on top of another. The gate itself is a rectangular room from which one enters the town by another threshold. In the Israelite period this room had a bench against its western wall: this recalls the biblical texts which tell how the elders of a town would sit at the gate to settle the affairs of the community.

A drain passes underneath the gate from the inside of the town (Pl. XXVI). It is built of stone and covered with stone slabs. On its way out, it cuts across the last level of the Early Bronze, then across the fallen bricks of the great wall and finally those bricks which had stayed in position beneath the Middle Bronze gate. The drain has been preserved only for a short distance inside the town, after which it is completely destroyed.

On the other hand, the Middle Bronze levels have suffered badly almost everywhere. There are very few houses of which the complete plan has survived, and they are not very interesting.

This is regrettable, for an important building certainly existed in the area excavated. But only one room of it remains. It was built with strong walls which were dressed only on the inside, and which reach down through several levels of the Early Bronze, Clearly, it was an underground room connected with a building above which has been entirely destroyed. It was probably vaulted in corbelling, for large slabs of stone have been found among the

ruins and one of them had fallen to the ground. The pottery dates from the 17th and 16th centuries B.C. After the collapse of the ceiling, there was a deep depression in the ground which was filled in with rubbish, mainly of the Late Bronze.

What was this underground construction ? The absence of watertight coatings shows that it was not a cistern; the absence of bones and funeral offerings proves that it was not a tomb, and the care devoted to its building proves that it was something more important than a storehouse. Could it be a sanctuary ? This hypothesis is supported by two pieces of evidence: against the western wall stands a narrow bench, upon which several little jugs were found, and in the south-east corner is a cavity surrounded by large stones. The seat would be the place where the offerings were placed, and the hole the *favissa* where they were subsequently thrown.

This hypothesis has been confirmed by the excavation of the lower levels. The room, we now know, saw three successive stages. To begin with, it was rectangular, with walls and floor covered with a lovely yellow coating. Against the western wall stood a small bench, as in the latest period. In the south-east corner, under the cavity of the later construction, a great jar was buried up to its shoulder, within which were two vases from the first part of the Middle Bronze and the fragile bones of the foetus of a pig. The bones of a young pig, some three or four months old, were found against the northern wall, with a little pottery. This underground sanctuary, where young pigs were sacrificed, is obviously of the highest interest, and it is to be regretted that, when discovered, it was so ruined and so empty.

Shortly afterwards, the south wall of the sanctuary threatened to collapse; it was therefore buttressed by a large wall of crude brick which followed the line of the construction but stopped short of the south-east corner to give access to the buried jar.

This brick wall collapsed in its turn. It was then—still during the Middle Bronze—that a new level was established on the ruins and that the sanctuary was reconstructed and enlarged towards the south; this is its last condition, which we have already described.

The large tombs of the southern Chalcolithic cemetery were almost all re-used during the Middle Bronze. Some tombs from the same period have been found on the Tell itself. Outside the area occupied by the Middle Bronze town, there are several tombs of adults amid the ruins of the Early Bronze town, which covered, as has been said, the entire hill. One of these tombs contained three vases of the 19th or 18th century and the skeleton of a woman (so at least we think), holding her little child, who died with her.

Inside the Middle Bronze town, tombs of adults are never found, though very young children were often buried under the floors of houses, usually in a jar containing some little vases as offerings. These tombs should not be interpreted as foundation-sacrifices. There is, however, one instance where this interpretation is probable: we discovered, inside the stone-work of the gate,

a jar containing the skeleton of a new-born child. This reminds us of the text of I Kings xvi. 34: Hiel rebuilt Jericho; he laid its foundations at the cost of his first-born, and set up its gates at the cost of his youngest son.

The Late Bronze Age.

The late Bronze levels, dating between 1550 and 1200 B.C., have been as badly destroyed as those of the Middle Bronze. The most interesting building we cleared comprised, it seems, a courtyard leading into a long room, or perhaps a hall with a colonnade. What remains is of very poor architectural worth, but one object found there allows us to conclude that it was perhaps a building used for religious purposes.

It is a little statue, four inches high. It is made of bronze, covered over with silver leaf, which has partly disappeared. It represents the goddess Hathor, with the curly hair-style which is her characteristic: a hole in the top of the head marks the place where the horns which completed her head-dress were inserted. She is clothed in a dress with shoulder-straps, a dress which reaches down to her ankles. She was holding her arms out in front of her, but both of them have disappeared; the eyes were of a white paste, of which a tiny fragment remains. The form is delicate, and the details are rendered with precision. It is probably of Syrian make, and is one of the most beautiful figures ever found in Palestine. Several other female figures, in terra cotta and of a coarser workmanship, were found in the region and seem to confirm the view that a temple stood in the area.

The Israelite Levels.

The Israelite levels are, happily, far better preserved. Immediately above the ruins of the Late Bronze, houses appear which stand in orderly lines along well-marked streets.

The type of building is characteristic. The walls have only one line of stones, and some of them are composed of pillars, the spaces between which are filled up with rough stones; it was a quick and cheap way of building a wall strong enough to support a first floor.

These houses were several times repaired or rebuilt on the same plan. The street level rose gradually by about eighteen inches. This indicates a long occupation, which was uninterrupted by large-scale destructions.

The objects found in the houses naturally belong to the last phase of this occupation. It was brought to an end by a catastrophe: the houses were pulled down and burnt, and their furniture left on the spot. A vast amount of pottery has thus been preserved, all dating from the turn of the 10th and 9th centuries B.C. This date is confirmed by several seals, and by a little model sanctuary, unhappily broken and thrown into a hole along with the broken potsherds of this level. It is similar to several other objects found in Cyprus, at Megiddo and in Transjordan. The general date is the 10th or 9th century B.C. These are pious household objects of the first Israelite period.

A fairly extensive clearing of this level at several points on the Tell gives us some idea of the development of town life. The houses are grouped with their backs to each other along parallel streets. The plan is always the same: from the street one enters a courtyard, on each side of which stand two rooms, and at the end of which are one or more rooms. All the houses follow more or less the same arrangement and have roughly the same dimensions. Each represents the home of an Israelite family and the very uniformity of the dwellings shows that there was no great social inequality among the inhabitants. This corresponds excellently with the social conditions of the period under David and Solomon and the first kings of the northern dynasty. The following period will offer a striking contrast.

This level was brutally destroyed and, in at least one part of the excavations, new buildings rose on the site of the ruins. Cutting through what remained of the previous walls, running across courtyards and over streets, new walls were founded on the soil of the level which had been destroyed. One building has been found (Pl. XXIV, 2) where the plan is the same as in the previous houses, but it is built on a different orientation and is flanked by a great hall with pilasters. In front of it there is a wide passage, and then the beginning of another large building.

The construction is far superior, the walls are larger, built with two lines of stones, and the cheap system of packing the space between pillars by rubble has disappeared. The corners too are solidly knit together by carefully dressed stones.

It was an important building which had been undertaken, for what remains—and that to a height of three feet—represents no more than the foundations. In fact, the walls of the preceding period still exist to the same height, the new walls have no doors, and one can still recognize, on their top, the places destined to received the thresholds; everything had to be filled in to this height. But the building was never finished. Not only is there no trace of its superstructure and no archaeological level to which we could trace its occupation, but there are several indications that work was interrupted. Near the north-east corner a great dressed stone stands on a ground level belonging to an earlier era. It did not fall from the corner because this corner is part of the foundations and, on the hypothesis that the building was finished and destroyed afterwards, the stone would not have fallen so low. It was made to be set on the corner but never put in place. Outside the building several large blocks of stone still untrimmed stand in line and beside them was a heap of lime. One had the impression that here was a workyard where work was suddenly stopped. We shall see later what historical conclusions can be drawn from these observations.

After this interruption, Tell el-Fâr'ah was abandoned, it appears, for a short time. Then life returned to the hill, though in a very small way. We have unearthed the remains of walls which rose above the destroyed level of the 10th–9th century and which are later than the unfinished building whose

foundations go deeper. Against these walls several groups of pottery have been found; they are of the 9th century. This level is poorly represented. It corresponds to a short and none too populous occupation.

Occupation is far more dense on the following level. At this epoch, there was a large building just near the gate. It consisted of a closed courtyard from which one entered a house of many rooms up a flight of steps. One very narrow room must have formed the well of the staircase which led to the first floor. At the back, a large room contained the debris of about one hundred and fifty jars, and there were several other constructions round about.

The dimensions and the plan of this building, its position near the town gate, the storehouse full of jars, all indicate that it was not an ordinary dwelling-place, but a public building, the residence of the governor and the centre of administration.

Elsewhere we find a group of very lovely private houses (Pl. XXIII, 2). They follow the plan of the 10th century houses—a courtyard with two rooms on either side and one or more apartments at the end. They are, however, a little larger and the construction is more careful: the plan is more regular, the walls are with two lines of stones, the stones better trimmed, the corners strong.

This rich quarter is separated by a long straight wall from a poor quarter where smaller houses are huddled together and where the techniques of an earlier age perpetuate themselves in a steady decline.

The wealth of pottery collected in the houses of both rich and poor and in the administrative building is very similar to that of Samaria and closely resembles that found at Megiddo during the same period. But it is distinguished by certain characteristics from that belonging to the south of Palestine; we have therefore two archaeological regions, corresponding to the two political kingdoms. Comparison leaves no doubt about the date of this level: it belongs to the 8th century B.C. The juxtaposition of rich and poor houses, which contrasts with the uniformity of the dwellings in the 10th century, reflects a social evolution—the birth of an urban proletariat. It is a concrete illustration of the invectives of the prophets of those days against the luxury of the houses which the rich built out of the money they had extorted from the poor. Amos v. 11: " Since you trample down the poor . . . and have built houses of carefully dressed stone, you shall not dwell in them ", or Hosea viii. 14: " Israel has forgotten its Maker and built palaces . . . and therefore I shall unleash a fire against its towns which shall burn down its palaces ".

The prophecy was fulfilled. The great building and the splendid houses of Tell el-Fâr'ah were destroyed and burnt, and we can date the event exactly. Above the ruins and ashes rose a new level, much poorer. The administrative building was re-occupied, but secondary walls disfigure its plan. In these new installations, we find pottery which is very clearly characterized by its paste, workmanship and shape. It has been found also, in the same region as Tell el-Fâr'ah, at Samaria and at Dothan, in one isolated site in the south

1. Chalcolithic Tomb in the northern cemetery.

2. Private houses of the 8th century B.C.

1 The Early Bronze Potter's Oven.

2 The unfinished building, looking to the south.

The 1st Early Bronze Levels.

The Middle Bronze Drain.

(Tell Djemmeh) and in a tomb at Amman. It is not of Palestinian origin, but is identical with that which the present excavations at Nimrud are bringing to light in the palace of Sargon II. It is Assyrian pottery, brought to Tell el-Fâr'ah by the conquerors or by the colonists they installed there. The authors and the date of the destruction of the fine Israelite level are thus determined: Tell el-Fâr'ah was captured by the Assyrians when they laid siege to Samaria in 723 B.C.

The town was then dismantled. Life continued, but never again did the inhabitants know the same level of prosperity. Rather, it was a period of slow decadence, and about 600 the site was finally abandoned. One could doubtless invoke, as the reason for this abandonment, the general impoverishment of Palestine under the foreign overlordship of the Assyrians, the Chaldeans and the Persians. But this historical explanation does not seem sufficient and (once more) it is probable that another outbreak of malaria led the population to emigrate.

Tell el-Fâr'ah and Tirzah.

One question is still to be answered: what biblical site should we identify with this town, which knew so long a history and which, from being a royal town of the Canaanites, became an important city in Israel ? Among the identifications proposed before the excavations, Albright's suggestion seems to have been proved right by our discoveries: Tell el Fâr'ah must be the ancient Tirzah.

The Bible gives us only very meagre information about Tirzah. It appears at the end of a list of royal Canaanite cities conquered by Joshua, Jos. xii.24. It became the capital of the Northern Kingdom after Shechem[1]. This change took place under Jeroboam I, according to a reference in the history of the death of Jeroboam's son, I Kings xiv.17. It is true that here the Greek version does not support the Hebrew text, but its tradition must give way to that of the Massorah.[2] In any case, Tirzah is mentioned as a royal residence under Baasha, I Kings xv.21 and 33, and remained such until the founding of

[1] I Kings xii. 25. According to the same verse, Jeroboam left Shechem and fortified Penuel. Hence it is sometimes said that Penuel too was a capital city. But the text refers rather to a military expedition to re-establish Israelite control over Transjordan just after the invasion of Shoshenq.

[2] The oldest form of the Septuagint did not contain I Kings xiv. 1-20. It gave the equivalent in the long addition of I Kings xii. 24^{a-z}. The story is then told before the schism and consequently Jeroboam and his wife are living at Ṣereda, written Σαρειρα as elsewhere in the Greek. The hexaplar text reintroduced the story of I Kings xiv. according to the Hebrew text, but in verse 17 Σαριρα is influenced by the addition of chapter xii. This addition has no independent historical value. As regards the story of the death of Jeroboam's son, it is consistent with itself in suppressing all allusion to the royal character of Jeroboam and to his faults, but it contradicts itself by retaining the threats against his descendants, for these presuppose that the religious and political schism is already a *fait accompli*, and, presented thus, the story loses all its meaning.

Samaria by Omri, I Kings xvi.23–24. Tirzah was the northern capital for less than half a century.

The transferring of the seat of government to Samaria did not mean the end of Tirzah, for it is mentioned one hundred and fifty years later, with reference to the usurpation of Menahem, II Kings xv. 14 and 16. Verse 14 is simple enough: Menahem, leaving Tirzah, went to Samaria and seized the throne. But verse 16 is very obscure: " Menahem sacked Tipsah, with all those who were in it, from Tirzah, because they had not opened to him". Tipsah, which is Thapsacus on the Euphrates, is an absurd reading, and, according to the Lucianic recension, we should read Tappuah. Since Tappuah must have been on the frontier between Ephraim and Manasseh, south of Shechem,[1] Tirzah should lie in the same region, and not at Tell el-Fâr'ah.[2] But the mention of Tirzah in this verse is not above suspicion,[3] and it is quite possible that "from Tirzah" is a clumsy repetition of the same words in verse 14. Besides, there are other difficulties in this same verse 16, and a text so corrupt cannot be used to identify Tirzah.

On the other hand, we know that Tirzah lay inside the territory of Manasseh and belonged to the clan of Hepher, Num. xxvi.33; cf. xxvii.1; xxxvi.10; Jos. xvii.3. But the "land of Hepher" formed part of the third prefecture of Solomon, which was situated, it is thought, in the plain of Sharon.[4] Here again we are far from Tell el-Fâr'ah. Note, however, that the genealogy of Hepher is very complex. Tirzah is one of the "daughters" of Zelophehad, who is only one among many sons of Hepher. These "daughters" had been left without any inheritance, Num. xxvii. 1 ff. but in the end they were assigned a portion in the division of Canaan, Jos. xvii.3–4. It is possible that this portion was not the same as the "land of Hepher". Now, among the other "daughters" of Zelophehad, two, Noah and Hoglah, are places mentioned in the ostraca of Samaria. These documents tell us nothing more about Noah, but they do tell us that two villages, Yaṣit and Geba', were dependencies of Hoglah. Now, the latter are quite certainly represented by the two modern villages of Yaṣid, four miles west of Tell el-Fâr'ah, and Djeba', eight miles north-west of Tell el-Fâr'ah. The last

[1] Jos. xii. 17; xvi. 8; xvii. 7-8. Probably to be identified with Sheikh Abu Zarad, cf. Abel, Géographie de la Palestine, II, p. 475.

[2] This is why Abel proposes, for Tirzah, the site of Djemma'in, to the north-west of Sheikh Abu Zarad, *Géographie de la Palestine*, II, p. 485.

[3] If this is meant to tell us where Menahem started out from, one would have expected rather Samaria, according to verse 14. But if it is meant to tell us where he began his work of destruction, one would expect a second place-name to tell us where he stopped. Moreover, this presupposes that Tirzah formed part of the territory of Tappuah, which is improbable.

[4] See especially A. Alt, *Palästinajahrbuch*, XXVIII (1932), p. 27 f.; B. Maisler, *Zeitschrift des deutschen Palästinavereins*, LVIII (1935), p. 82. Albright's attempts to localize north-east of Shechem the " land of Hepher " (*Journal of the Palestine Oriental Society*, XI (1931), pp. 248-251) are not convincing.

"daughter" of Zelophehad, Tirzah, must therefore lie in the same region, and the site of Tell el-Fâr'ah suits it perfectly.

Archaeology confirms this identification, and it is possible to read in the different levels of the Tell all the history of Tirzah as told by the Bible. Tell el-Fâr'ah, we said, was a royal town of the Canaanites: the king of Tirzah was defeated by Joshua, according to Jos. xii.24, and the destruction of the Late Bronze level at Tell el-Fâr'ah would mark the capture of the town by the Israelites. They occupied it for a long period in peace—which is our third level. But, at the beginning of the 9th century, this level is overturned; we know from the Bible that in 885[1] Omri laid siege to Tirzah and took it, and that the usurper Zimri perished in the flames of the palace, I Kings xvi. 15–18. Omri reigned in Tirzah for six years, I Kings xvi.23; the first four years, however, were taken up with the struggle against his rival Tibni.[2] It was only after he had made his position secure that Omri could think of building at Tirzah: they are the buildings whose foundations reach down into the third level. These same buildings, however, were never finished, for two years later, in the sixth year of his reign, I Kings xvi. 23–24, Omri took the decision to leave Tirzah and to found a capital on the hill of Samaria which he had just bought. The buildings where we found work suddenly stopped are a vivid illustration of this transfer. The royal family, the officers and soldiers with their families and their servants all left with the king. It is probable that many inhabitants of Tirzah, workmen and merchants, also left with the Court. It is even possible that Omri, since he was going to found his capital on a deserted hill, took with him all or part of the population of Tirzah. This would explain the poverty of our intermediate level of the 9th century, preceded perhaps by a short period of complete abandonment. However, during the period of prosperity in the Kingdom of Israel, under Joash and Jeroboam II, from 798 to 743, Tirzah must have recovered some of its former life and it was from there that Menahem marched to attack Samaria in 743. This flourishing period is that of our 8th century level, with its splendid private houses and the official building, perhaps the residence of Menahem if the latter was actually governor of Tirzah. The town was taken by the Assyrians when they invaded Israel and laid siege to Samaria in 723; this would represent the destruction of our second level, which is dated exactly by the Assyrian pottery which we find in the next level.

Since the history of Tirzah is so closely linked with that of Samaria in

[1] This is the chronology I adopted in *Les Livres des Rois* (Bible de Jérusalem), 1949, which is also adopted by Montgomery in his commentary (1951). Albright's dates, for this period, are about ten years later.

[2] The ten years of Omri, I Kings xvi. 23, should be counted from the time of Zimri's usurpation, in the 27th year of Asa of Judah, I Kings xvi. 15. The 31st year of Asa, I Kings xvi. 23, marks the beginning of Omri's unchallenged rule after the final defeat of Tibni, I Kings xvi. 22; hence the struggle must have lasted for four years. This is confirmed by I Kings xvi. 29: the end of Omri's reign, twelve years, corresponds to the 38th year of Asa.

our biblical texts, it is interesting to see whether the same connections can be found between the archaeology of Tell el Fâr'ah and that of Samaria. The pottery of Samaria will very soon be made known by a publication of Miss Kathleen Kenyon, who has most kindly lent me her manuscript. Our third level, which comes to an end at the beginning of the 9th century is naturally not represented at Samaria, which was founded only in 880. On the other hand, the first two periods of Samaria, which run from 880 to 850, have no corresponding period at Tell el-Fâr'ah. Contact begins only during the third period of Samaria (850–800) and our intermediary level of the 9th century. But from Samaria IV and V and especially during Samaria VI, that is, from 800 to 722, there is a perfect parallel with our Level II: the pastes and the shapes are identical for the two sites so that one could almost exchange the plates of the two publications. The same parallelism continues between the VIIth period of Samaria and the upper level of Tell el-Fâr'ah, which are later than the fall of Samaria; Assyrian pottery is found on both sites. This last test seems decisive: not only does the archaeology of Tell el-Fâr'ah fit in with the history of Tirzah, but it dovetails into the archaeology of Samaria in the same way as the history of Tirzah dovetails into the history of Samaria. Apart from a written text to give us apodictic proof, it seems that archaeology has provided perfectly convincing arguments for the identification of Tell el-Fâr'ah with Tirzah.

If this conclusion is justified, it becomes possible to tackle in a new light a problem which has vexed historians: Why did Omri leave Tirzah for Samaria? Why did he establish his new capital on a previously uninhabited site, and why on that particular one? In a recent work, A. Alt[1] gives the following answer. Omri had to rule a kingdom where Israelites and Canaanites were living side by side. Samaria belonged to a territory where the Canaanites were still numerous and the hill was probably owned by the Canaanites. Omri bought it outright for his own personal property, provided it with a population entirely (or at least principally) Canaanite, and thus ensured that the town had no connection with Israel's past, as had Shechem and, to a lesser degree, Tirzah. Omri was perhaps thinking of making Samaria the capital of the Canaanite element in his kingdom, while maintaining another capital for the Israelite element. This in fact came to pass under his successor Ahab, when Samaria became the Canaanite capital and Jezreel the Israelite capital.

It is impossible to discuss this new thesis in detail, with all its historical, political and religious implications.[2] Suffice it to say that, on one very important point, it does not fit in well with our archaeological findings. It is a gratuitous assumption to say that Samaria was a Canaanite possession, since we know nothing about the site before Omri bought it. It is still more

[1] A. Alt, *Der Stadtstaat Samaria* (Berichte über die Verhandlungen der sächsischen Akademie der Wissenschaften in Leipzig, Phil.-hist. Klasse, Bd. 101, H. 5), 1954.

[2] Cf. my review in the *Revue Biblique* LXIII (1956), pp. 101-106.

risky to presume that Omri installed a Canaanite population in his new capital. We have seen that soon after the departure of Omri and his court the population at Tirzah-Tell el-Fâr'ah either left the site entirely, or at least fell sharply in numbers. It is reasonable to suppose that they went to Samaria to people the new capital. But in that case Samaria was not, as Alt would have it, a Canaanite town, nor destined to be the capital of the Canaanite part of the kingdom.

It is true, however, that by buying the uninhabited hill of Samaria, Omri wished to establish his dynasty on neutral ground, from which it would be easier to govern a kingdom as yet far from united. But I think this tension was not so much between the Canaanites and the Israelites as between the Israelites themselves. Tirzah had just witnessed a period of anarchy and bloodshed: after two years' reign, Elah had been assassinated by Zimri, after seven days Zimri had perished amid the flames of his palace, and for four years Omri had warred against Tibni, to whom half the people had rallied, I Kings xvi.8–21. Omri, by setting himself up in a new capital, hoped to escape from the factions which divided Israel. In the same way David had established himself in Jerusalem, which was his own conquest,and therefore a neutral territory from which he could govern both Israel and Judah and by-pass the dispute as to which provided the king.

The transfer of the capital from Tirzah to Samaria, and the choice of Samaria, must nevertheless be explained by considerations of external policy as well. Tell el-Fâr'ah faces east, towards Transjordan, where the Israelites had blood-brothers and where, under David, the opposition of Israel to Judah had been concentrated. It was a land where the kings of Israel controlled vast territories which were threatened by the Arameans, the Ammonites and Moabites. Tell el-Fâr'ah was therefore a suitable site for the capital of the kingdom of Israel in its first period. Its communications with the south, however, are less satisfactory, and it was there that the kings of Israel had to defend their frontier against the kingdom of Judah, I Kings xv.16–22, and against the Philistines, I Kings xv.27; xvi.15. Its communications were still more difficult with the west, where the richest part of the kingdom lay, and with the north-west, where the way to Phoenicia and the ports for foreign trade lay open. Omri had been acclaimed as king by the army when it was camped before the Philistine town of Gibbethon, I Kings xvi.15–16, and he did not forget the event. He wished also to lead Israel out of its isolation and to strengthen friendly relations with the Phoenicians, for the sake of trade and to protect himself against Aramean pressure which was already threatening in the east, I Kings xv.20. The marriage of his son Ahab with Jezebel of Tyre was meant to put the seal on this alliance. All this goes to explain why he chose as his new capital the hill of Samaria, which was easy of access and yet easy to defend—"proud crown, which the fertile valley wears upon its head", Isaiah will exclaim, xxviii.1. It was nearer than Tirzah to the centre of the kingdom, with excellent communications to the north, south and

west. Tell el-Fâr'ah looked eastwards, Samaria to the west. This contrast corresponds to the change in political orientation which marks the reign of Omri, and provides a last argument in favour of the identification of Tell el-Fâr'ah with Tirzah.

HURRIAN AND OLD ANATOLIAN PLACE NAMES IN THE SEMITIC WORLD: SOME TENTATIVE SUGGESTIONS

By B. S. J. Isserlin

Among the most tricky problems besetting the study of Ancient Near Eastern place names (a difficult branch of research in any case) is the question how to deal with place names evidently of non-Semitic character. This question is of importance also for those more particularly interested in the Semitic-speaking regions, for the possibility of place names of *e.g.*, Anatolian or Hurrian affinities occurring at some distance from their presumable home ground, *e.g.*, in Palestine or in the region of Ugarit, has been noted by scholars such as Borée or Alt some time ago.[1]

Now, in order to be convincing, an attribution of a place name to a linguistic group should conform to the grammatical structure and known vocabulary of whatever language may be in question: also, if sense can be made of the name, its meaning should be such as to be *prima facie* suitable as the designation of a place.

It seems to the writer that, in the case of *Hurrian*, an attempt can perhaps by now be made to deal with names of localities on such lines: the more so as there is at our disposition a detailed study by Gelb, Purves, and MacRae analysing the names of *persons* in a similar fashion.[2] It would also seem that the natural region to start with should be the neighbourhood of Kirkuk and districts adjoining, since Hurrian speakers are known to have lived there early in some numbers.

The types of place names which a study of the materials culled from the sources concerned suggests, seem to be the following:

(1) *Divine names* serving to designate places. This is a fairly frequent group: examples include ḫu-ur-ra,[3] til-la-a,[4] šu-a-la.[5]

(2) *Names consisting of a geographically significant term* with some formative element attached. Such are pa-pa-an-te[6] cp. pap "mountain"[7] and the ending -ant,[8] or ḫa-wur-ni-we,[9] cp. ḫawur "earth, low"[10] and endings -ni[11] and -we.[12]

[1] See W. Borée, *Die alten Ortsnamen Palästinas*, and A. Alt, *Z.D.P.V.*, LXVII, 1944-5, p. 113 ff, *inter alia.*

[2] I. J. Gelb, P. M. Purves, A. A. MacRae, *Nuzi Personal Names* (quoted hereafter as *G.P.M.*).

[3] C. F. Jean, *R.E.S.* 1941, p. 125, and *G.P.M.* p. 218.

[4] C. F. Jean, *R.E.S.* 1941, p. 127, cp. *G.P.M.* pp. 188, 266.

[5] C. J. Gadd, *Iraq* VII, 1940, p. 43, cp. *G.P.M.* pp. 188, 258.

[6] L. Oppenheim, *R.A.* XXV, 1938, p. 150.

[7] *G.P.M.* p. 243.

[8] *G.P.M.* p. 201.

[9] *G.P.M.* p. 215.

[10] *G.P.M.* p. 215.

[11] *G.P.M.* p. 238.

[12] *G.P.M.* p. 274.

(3) *Compound names.* These are structurally analogous to the names of persons, and are in fact probably as a rule the names of the founder or owner of a locality. We can distinguish

(*a*) *Nominal compounds*, such as kiparrapḫa[1] made up from kip[2] and arrapḫa,[3] or pu-ḫi-ma-li[4] consisting of elements puḫ[5] and mal.[6]

(*b*) *Verbal compounds*, as in ḫa-ši-pa-bu[7] comprising the elements ḫaš,[8] -p[9] and ab/p.[10]

(*c*) *Reduced compounds.* In these only one element is left and usually provided with an ending like -ia or -wa/we. Such seems to be, *e.g.*, mi-ni-ia-wi,[11] root element min.[12]

Other endings which deserve notice include -ḫa/ḫi, as in ḫa-ba-ru-ha,[13] -š, as in a-be-na-aš,[14] -n, and combinations of several such elements.

If we keep this state of things in mind and proceed now to a study of the regions at some distance from the Hurrian lands just studied, then we shall find indications of the existence of analogous place names firstly in northern Syria, in the region of Alalakh.

The place names in the tablets found at that city[15] include such forms as bi/e-ra-še-na,[16] a Hurrian personal name also known in Palestine,[17] ki-i-ki,[18] cp. Hurrian Kikki [19] or a-ri-en-na-še.[20] These come from texts of the New Kingdom, but older names from level VII (dating to the 18th century B.C.) include such items as a-gi/ki-e,[21] or ta-ar-ma-an-ni-e[22] which look like abbreviated compounds. There is nothing inherently startling in this, for Wiseman, when dealing with the names of persons from Alalakh, remarked that people with Hurrian affinities were present at the time of level VII in such strength that their arrival in the district was probably not very recent.[23]

[1] L. Oppenheim, *R.A.* XXXV, 1938, p. 146.

[2] *G.P.M.* p. 227.

[3] *G.P.M.* p. 205.

[4] C. F. Jean, *R.E.S.* 1941, p. 126.

[5] *G.P.M.* p. 246.

[6] *G.P.M.* p. 233.

[7] L. Oppenheim, *R.A.* XXXV, 1938, p. 154.

[8] *G.P.M.* p. 214.

[9] *G.P.M.* p. 241.

[10] *G.P.M.* p. 201.

[11] G. Contenau, *R.A.* XXXV, 1938, p. 26.

[12] *G.P.M.* p. 235.

[13] E. Ebeling, B. Meissner, E. F. Weidner, *Die Inschriften der altassyrischen Könige*, p. 53.

[14] L. Oppenheim, *R.A.* XXXV, 1938, p. 319.

[15] See D. Wiseman, *The Alalakh Tablets*, for the material.

[16] D. Wiseman, op. cit., p. 154.

[17] *G.P.M.* p. 245, cp. *Z.D.P.V.* XLIX, 1926, p. 325 ff.

[18] D. Wiseman, op. cit., p. 155.

[19] *G.P.M.* p. 226.

[20] D. Wiseman, op. cit., p. 154: cp. *G.P.M.* p. 203 (ari), p. 210 (enn).

[21] D. Wiseman, op. cit., p. 154: cp. *G.P.M.* p. 199 (akk) and p. 14 personal name a-ki-a-a).

[22] D. Wiseman, op. cit., p. 157: cp. *G.P.M.* p. 262 (tarm), p. 240 (-nni).

[23] D. Wiseman, op. cit., p. 9.

Proceeding now southwards towards Phoenicia, we have a possibly Hurrian place name in the Execration Texts, if i3 iy[1] can be treated as a shortened compound with ending -ia, and linked with the Hurrian personal name a-ri-a.[2] A more likely candidate is ḫbrḫ[3] at the northern extremity of Galilee, a homonym to the ḫa-ba-ru-ḫa mentioned above. We might further refer to šrḫn, biblical Sharuhen[4] which looks like the root element šarru " prince "[5] and the element -ḫ and -n: " princely place " would indeed be a most fitting designation for the town which was the mainstay of Hyksos power in Palestine. One might also refer to psps in Transjordan[6] and recall the Hurrian personal name pa-as-pa-su.[7] These references come from the period of the New Kingdom: there seem to be no cogent reasons obliging us to treat any Palestinian place name from a time prior to the age of trouble terminating the Middle Kingdom in Egypt, as Hurrian. This again seems to agree quite well with what is at present known of the general historical situation.

Turning now to Old Anatolian names the position is much more difficult, particularly for the non-specialist. It seems to the author worth asking whether some of the names from the Alalakh tablets could not be Anatolian derived: examples are ku-wa-an-ni,[8] cp. kuw(a)nna, Kyana,[9] kun-na, cp. -kuna, Konna,[10] or ku-la-an-te,[11] cp. kulã-ta, Kyllandos.[12]. In Palestine one might think of such names as Megiddo, Ma-gid-da[13] and particularly Lachish, la-ki-si[14] which recalls the Lakisi in Cilicia[15] and a number of place names formed from the element laka/laga in Anatolia;[16] the la-ak-ka in the Atchana letters[17] may go with this group. Lachish and Megiddo have both furnished some " Khirbet Kerak " ware in the course of excavations: In view of the Anatolian affinities of this ware it seems just possible that the names arrived at the same time as the makers of this ware, in the second half of the third millennium B.C.

[1] F. Posener, *Princes et Pays d'Asie et de Nubie*, p. 76 E 21.

[2] *G.P.M.* p. 202.

[3] J. Simons, *Handbook for the Study of Egyptian Topographical Lists*, list XXII, g. 11.

[4] J. Simons, op. cit., p. 125 doubtful, and with ḥ, not ḫ: but cp. M. Burchardt. *Die altkanaanäischen Fremdworte und Eigennamen im Agyptischen*, II no. 874 (with ḫ). Those who would like to derive the name from Semitic Sirḥan have to explain why this straightforward term should have been transformed into such an extraordinary and meaningless shape.

[5] For occurrence of šarru in Hurrian, cp. *G.P.M.* p. 250.

[6] B. Grdseloff, *Revue de l'Histoire Juive en Egypte*, I, 1947, p. 79.

[7] *G.P.M.* p. 312.

[8] D. Wiseman, op. cit., p. 155.

[9] J. Sundwall, *Die einheimischen Namen der Lykier* (Klio, Beiheft 11) p. 128.

[10] D. Wiseman, op. cit., p. 155, cp. J. Sundwall, op. cit., p. 123.

[11] D. Wiseman, op. cit., p. 155.

[12] J. Sundwall, op. cit., p. 121.

[13] *E.A.* 244, 24: cp. J. Sundwall, op. cit., p. 138 (Magidon, Magydos).

[14] *E.A.* 287, 15, cp. *E.A.* 335, 10 (la-ki-ši).

[15] E. Forrer, *Klio* XXX, 1937, p. 185.

[16] J. Sundwall, op. cit., p. 131.

[17] D. Wiseman, op. cit., p. 155.

The author would like, lastly, to deal with two place names from central Palestine of the New Kingdom. In the stele published by Badawy[1] there occur a mpsin and a ḫtytn. It might be possible to divide the first of these names (neither of which seems easily to offer itself to interpretation from the Semitic side) into a stem mps, and an ending -n. One might similarly divide ḫtytn into ḫty, and -t -n. In the latter case, the t might stand for a sibilant of the z/ž class (as it apparently does in the case of tndr, corresponding to zi-in-za-ar, modern Zinjar[2]: and we should then have an-n and a -z/ž suffix, of types known in Old Anatolian,[3] after the main stem. But this main stem is rather startling, for ḫt, ḫty is the usual Egyptian transcription for " Hittite ".[4] Similarly the mps of the former name recalls the Mopsos, Mopsou-estia in Anatolia, a name element of long standing there.[5] The whole matter is rather speculative: but one is perhaps allowed to wonder whether there may not be some connection here with the Hittites mentioned by the Bible among the pre-Israelite inhabitants of Palestine, and who up to now have proved so remarkably elusive ? A " Hittite town " and a " Mopos town " in the region of Samaria would certainly be interesting, if their existence could be more definitely established.

[1] A. Badawy, *Die neue historische Stele Amenophis* II. *Annales du Service des Antiquités de l'Egypte*, XLII, 1943, pp. 1-23, esp. p. 17.

[2] J. Simons, op. cit., I, 173; *E.A.* 53, 42; cp. W. F. Albright, *Vocalisation of the Egyptian Syllabic Orthography*, p. 47 IX E 7.

[3] Cp. J. Sundwall, op. cit., pp. 40-1.

[4] Cp. examples listed by J. Simons, op. cit., p. 212.

[5] Cp. J. Sundwall, op. cit., p. 156 and Barnett, *Anatolian Studies*, III, 1953, p. 88.

Note.—R.A. stands for *Revue d'Assyriologie et d'Archéologie Orientale*; *E.A.* stands for *The Armarna Letters*, no. and line.

REVIEWS AND NOTICES

Jerusalem de l'Ancien Testament. Vol. II. By L.-H. Vincent, O.P., with the collaboration of A.-M. Stève, O.P. Quarto. Pp. xiii–xvii, 373–809. Text Figs. 111–214; Pls. CI–CXLIX.

Pere Vincent's work on Jerusalem of the Old Testament has been planned in three parts: the archaeology of the city, the archaeology of the temple and an historical synthesis. The present volume contains the second and third of these parts.

As all readers of this Quarterly know, the author is preeminent amongst living scholars in having personally witnessed the unfolding of the archaeological scene in Palestine for more than half a century. Combining that unique experience with a profound scholarship in all matters touching on the history and monuments of the country, he writes of Jerusalem with an authority and mastery unmatched at the present day.

Following the method already demonstrated in the first volume, the investigation of each topic begins with the literary sources, which are then confronted with the facts. The discussion of the temple is based on this procedure, four separate chapters being allotted to the temples of Solomon and Herod, of Ezekiel and the Mishnah respectively. This is sound method, for it separates the historical from the visionary and avoids the fallacy of supplying gaps in the basically factual accounts of the chroniclers and of Josephus with details selected, however discreetly, from the idealisations of Ezekiel and the rabbis.

Much of the difficulty in reconstructing Solomon's temple arises from the omission of essential dimensions by the writers of Kings, in particular the thickness of walls. To supplement the texts scholars have had recourse to build up a very comprehensive reconstruction of the shrine, given in plan, section and elevation on Pl. CI.

Beginning with the long section and applying to the measurements of I Kings vi three different triangles, the "sacred" Pythagorean triangle, the "Osirian" (H = $\frac{5}{8}$ Base) and an isosceles triangle whose height equals its base, Vincent arrives at a solution giving a uniform thickness of 5 cubits for all walls and a common factor of 5 cubits for all horizontal dimensions. For the front elevation and transverse section (Fig. 113) the equilateral triangle alone is used, based on the 5 cubit unit; and this serves not only to establish the proportions obtaining between horizontal and vertical dimensions, but also to support the restoration of a very convincing pair of towers, which flank and overtop the porch and house the staircases.

It is clear that the geometrical method is very largely speculative and could be used to support widely varying solutions, for none of which could

demonstrative value be claimed. From an architectural and logical standpoint Pere Vincent's final result appears to me to be eminently satisfactory and an acceptible interpretation of the facts supplied by the texts. But as a demonstration of method it is, perhaps, not quite so convincing. The plausibility of the scheme is, perhaps, only slightly impaired by the multiplicity of the triangles (four) on which it relies; but a more serious defect arises, as it seems to me, from the fact that an equilateral triangle of side 60 cubits does not stand 50 cubits high, as the theory requires, but very nearly 52. Consequently, to obtain agreement between the long section and the façade, Fig. 113 has had to be drawn with all its supposedly equilateral triangles depressed by nearly 4 per cent. of their proper height. Although we may feel some doubt whether the ancient builder would have accepted such an approximation as perfectly harmonious, I cannot find that the author makes any allusion to the difficulty.

For Herod's temple the principal source is necessarily Josephus, whose accounts in Wars V and Antiquities XV are subjected in chapter XV to an examination based on the sound principle of assessing the historian's purpose and defining his data rather than of attempting to reconcile his inconsistencies, and on the assumption already proved in the first volume that Josephus was a good eye-witness but poor archaeologist, and no better informed than ourselves on matters of antiquity.

So radical has been the destruction of the temple that only the outer enclosure wall with its gates, and parts of the rocky substructure remain to supplement the literary data. A chapter of outstanding interest, chapter XVIII, is devoted to the archaeological facts and the problems they raise. How much of the great enclosure wall is Herod's and how much the work of Solomon or his successors? Readers of the Quarterly will specially appreciate the author's warm tributes to Sir Charles Warren and the Ordnance Survey, on whose work all study of the problems depends. Some will be surprised to find that Vincent's answer to those questions is substantially the same as Warren's: all known sections of the perimeter wall in which the smooth-faced rebated masonry reaches the rock (in particular the S.E. corner and part of the west wall between Bab se-Silsileh and Barclay's Gate) are the work of Solomon or his successors; all those parts in which smooth-faced rebated masonry surmounts roughly bossed foundations are Herodian (p. 584–5).

The existence of these two classes of walling is a fact established by Warren's survey; their classification is chronologically distinct and their attribution to Solomon and Herod respectively, with all that that implies for the original emplacement of the Solomonic temenos, for the effect of Herod's expansion, and for the virtuosity of Iron Age masons, is a conclusion of such exceptional interest that it deserves a critical examination.

We start with the S.E. corner. There, directly implanted on the rock, Warren found five courses of gigantic masonry (Pl. CXV, g–c) characterized

by rebated margins of varying width and varying disposition on two, three or four sides of difierent stones. The wall above those five courses was distinguished from them by regular margins of a decorative character on all four sides of the stones.

For Vincent the dating of this corner is decisive (p. 583) and it becomes the chronological touchstone for the rest of the enclosure. He attributes the whole corner, from its foundations up to the " great course ", to the Solomonic period, and for three reasons: by analogy with the Iron Age masonry at Megiddo and Samaria; by the presence of eight Hebrew stamped jar handles above a stratum of virgin clay in which the foundations were sunk; and by the existence of an Iron Age juglet found intact in a pocket of rock within a metre of the angle.

I venture to doubt if this attribution will be found generally convincing. We cannot now inspect the masonry in question but scale alone sets it in a class far apart from anything to be seen either at Samaria or Megiddo, and comparison with the " Solomonic " tower shown in Pl. CXXXVII can hardly be said to support the identification. Warren could not observe and has not recorded, the stratification; but from the facts reported I suggest it is not permissible to infer that either the accumulation of Iron Age pottery or the deposit of the juglet took place after or at the time the foundations were laid.

Vincent's view requires us to distinguish two lots of masonry, both comprising vast areas of wall surface and both described as " *à refends et champ lisse* "; one we must attribute to Herod (N.E. and S.W. corners), the other to Solomon (S.E. corner and Wailing Wall area). Nowhere are the Solomonic characteristics of the second lot defined; indeed, at the S.E. corner courses b–G are clearly distinguished (p. 554) from their supposedly Iron Age foundations. Furthermore, their surface treatment, as described by Warren, is identical in every detail, as Vincent on p. 555 recognizes, with the Herodian masonry at Hebron. This is clearly an embarrassment, and it is only re-emphasised, in connection with the Wailing Wall (also Solomonic by the criterion accepted) by an appeal on p. 583 to " *quelques retouches hérodiennes* ".

The eastern wall of the Solomonic temple was doubtless, as Vincent on p. 634 describes it, a northward extension of the wall of the city. But the wall of the present enclosure, as Pl. CXV clearly shows, was nothing of the kind: its south end forms a corner without liaison with any contemporary or earlier structure to the south (for the wall shown abutting on courses a–N is manifestly later). On all grounds, therefore, I find it hard to believe that the S.E. corner of the Haram as it stands is anything but a Herodian structure, cut down in the 1st century through the Iron Age refuse and through any earlier structures on its emplacement. Recent road works have exposed several more courses than were lately visible and have made a further sounding very easy. It is much to be hoped that the

opportunity will be taken to elucidate the stratification at the foundations of this vital and now accessible point of the temple enclosure.

Space allows only the briefest appreciation of the third and last part of the work, a swiftly moving and masterly survey of the evolution of the city from the earliest times, seen as the background of the Old Testament narratives and woven together from the literary and archaeological data assembled and sifted in the preceding sections. A judicious extension of the scope of the work beyond the chronological limitations of the Old Testament enables the author to devote three valuable chapters to the last flowering of the city under Herod the Great and its final fall. Of particular interest in this section is the last chapter of all, describing that brief and dimly illuminated interlude after 70 A.D. in which the fatal illusions were bred and hardened which brought the city and its people to destruction.

In the two volumes of this monumental work all who are interested in the city and history of Jerusalem will find an assembly of the literary and archaeological data incomparable in the comprehensiveness of its scope, the clarity of its exposition and the authority of its criticism.

R. W. HAMILTON.

The Committee have to acknowledge with thanks the following:—

Al Machriq. Le Annee, Nos. 1 and 2, 1956.

American Journal of Archaeology. Vol. 60, Nos. 2 and 3, 1956.

American Journal of Philology. Vol. LXXVII, Nos. 2 and 3, 1956.

Les Annales Archeologiques de Syrie. Tomes IV and V, 1954-1955.

Annual of the British School at Athens. No. L, 1955 and Annual Report of Managing Committee for Session 1954-1955.

Antiquaries Journal. Vol. XXXVI, Nos. 1 and 2, 1956.

Bible Lands. Vol. XIII, Nos. 10 and 11, 1956.

Biblica. Vol. 37, Nos. 1 and 2, 1956.

Biblical Archaeologist. Vol. XIX, Nos. 1 and 2, 1956.

Biblical Research Monthly. Vol. XXI, Nos. 3–9, 1956.

Bulletin of the American Schools of Oriental Research. No. 142, 1956.

Bulletin of the Israel Exploration Society. Vol. XX, Nos. 1–2, 1956.

Bulletin of the University Museum, Philadelphia. Vol. 20, No. 1, 1956.

Cyprus, Dept. of Antiquities Reports. 1955.

Expository Times. Vol. LXVII, Nos. 5–12, 1956, and Vol. LXVIII, No. 1, 1956.

Harvard Theological Review. Vol. XLIX, Nos. 1–3, 1956.

Internationale Zeitschriftenschau fur Bibelwissenschaft und Grenzgebiete. Band III, Heft 1–2, 1954/1955.

Israel Exploration Journal. Vol. 6, Nos. 1, 2, and 3, 1956.

Jewish Quarterly Review. Vol. XLVI, Nos. 3 and 4, and Vol. XLVII, No. 1, 1956.

Journal of the American Oriental Society. Vol. 76, Nos. 1 and 2, 1956.

Journal of Semitic Studies. Vol. I, Nos. 1-4, 1956.

Kush. Vol. III, 1955.

Miscellanea Comillas. Vol. XXV, 1956.

Mittheilungen des Deutschen Archaeologischen Instituts, Athens. Band 68, 1953.

Nea Zion. Jan-Apr. and May-June, 1956.

Nouvelle Revue Theologique. Vol. 78, Nos. 3-8, 1956.

Orientalia Suecana. Vol. IV, 1955.

Revue Biblique. 63rd year, Nos, 2 and 3, 1956.

Scripture. Vol. VIII, Nos. 2-4, 1956.

Sudan Antiquities Service Occasional Papers. No. 3, 1955.

Syria. Vol. XXXII, Nos. 1-4, 1955.

United States Museum Annual Reports. 1955.

Die Welt des Orients. 1956.

Zeitschrift des Deutschen Palastina-Vereins. Vol. 71, No. 2, 1955.

PALESTINE EXPLORATION FUND.

ACCOUNTS AND REPORT FOR 1955.

At the Annual General Meeting held on 21st July 1955 the Chair was taken by Sir Ronald Storrs, whose distinguished career included the office of Governor of Jerusalem in the early years of the Mandate, 1917 to 1926. Having enjoyed his friendship for over fifty years I was one of the many by whom his death, some three months later, was deeply regretted. Mr. Norman Bentwich has paid a tribute to his memory in the Palestine Exploration Quarterly.

The past year has unfortunately been marked by the retirement, for various reasons, of all the officers of the Fund, though Canon Mortlock has since accepted office as Vice-Chairman and been succeeded as Hon. Secretary by Dr. Waechter. Professor Driver as Chairman, Professor Winton Thomas as Vice Chairman, Canon Mortlock as Hon. Secretary and Miss Kenyon as Hon. Treasurer have all rendered service of the utmost value to the Fund; as members of the Executive Committee they will, I am happy to think, continue to give their successors in office the advantage of their valued counsel.

I have also to announce with great regret that Professor S. H. Hooke wishes to resign the editorship of the Palestine Exploration Quarterly. It was in 1933 that Professor Hooke undertook the burden of editing the Quarterly Statement, and the Fund has every reason to be grateful for the great abilities, the patience and hard work which for twenty-three years he has devoted to a most successful editorship.

The third and final volume of the report on the excavations at Samaria-Sebaste by Mr. J. W. Crowfoot and Miss Kathleen Kenyon is to be published shortly. A number of generous donations towards the cost of this publication have been received or promised and it is hoped that it will not be necessary to realize any of the capital assets of the Fund for this purpose.

A course of four lectures was kindly organised by Miss Olga Tufnell:—

January 9th: The Qumran Community and the Early Church, by J. M. Allegro.

January 16th: Palestine and the Hellenistic World, by Professor C. M. Robertson.

January 23rd: Palestine in the Roman Period, by G. M. FitzGerald.

January 30th: Christianity in Egypt, by the Rev. J. M. Plumley.

In February we were glad to welcome Dr. Y. Yadin who gave an extremely interesting lecture on his first season of excavation at Hazor.

All these lectures are attended by members of the general public and it is somewhat disappointing to find that they seem to have little or no effect in bringing in more subscribers. Without an increase in the number of its members the Fund can scarcely take a proper share in its primary purpose, the exploration of Palestine.

G. M. FitzGerald,
Chairman.

31*st May*, 1956.

THE PALESTINE EXPLORATION FUND

BALANCE SHEET, 31st DECEMBER, 1955

1954		£ s. d.	£ s. d.
£	*Capital:*		
	Authorised—200 Shares of £1 each ..	200 0 0	
	Issued—9 Shares of £1 each, not called	—	—
	Surplus of Assets over Liabilities: (Excluding donated property)		
2,646	Amount as per last Balance Sheet ..	2,650 18 9	
5	*Add:* Excess of Income over Expenditure for year	194 8 9	
2,651			2,845 7 6
	Current Liabilities:		
302	Sundry Creditors for Current Expenses	396 8 4	
67	Subscriptions in advance	121 0 2	
—	Rent received in advance	62 10 0	
—	Donations toward cost of Publication	357 8 10	
			937 7 4
£3,020			£3,782 14 10

1954		£ s. d.	£ s. d.
£	*Fixed Assets:*		
	Freehold Property—Donated:		
—	No. 2, Hinde Street (estimated value £5,000)		—
	Alterations and Improvements:		
1,189	No. 2, Hinde Street at net book value 1st July, 1948		1,188 17 5
	Museum, Library and Storeroom, Fixtures, Fittings, Furniture, etc., at		
382	net book value 1st July, 1948 ..		382 2 7
	Typewriter at cost	39 10 0	
	Less: Depreciation to date	24 10 0	
18			15 0 0
	Jewish Colonial Trust—Donated:		
—	50 Ordinary Shares of £1 each, fully paid		—
1,589			1,586 0 0
	Current Assets:		
	Stocks of Publications, Maps, Books, Models, Lantern Slides, Library and		
—	Antiques		—
10	Subscriptions in Arrear	9 18 0	
	Investments—Post Office Issue—at cost—		
	£374 12s. 0d. 3½ per cent. Conversion Stock £400		
	£50 3 per cent. Savings Bonds 1955–65 50		
	£264 1s. 8d. 2½ per cent. Savings Bonds 1964–67 .. 250		
	£300 3½ per cent. Defence Bonds 300		
	£100 2½ per cent. Defence Bonds 100		
1,100	(Market Value £943) ——	1,100 0 0	
87	Sundry Debtors	48 5 4	
	Cash at Bank, on Current Account		
208	and in hand	1,012 3 4	
26	Cash on Deposit	26 8 2	
			2,196 14 10
£3,020			£3,782 14 10

AUDITORS' REPORT TO THE MEMBERS OF THE PALESTINE EXPLORATION FUND.

We have examined the above Balance Sheet, Receipts and Payments Account and Income and Expenditure Account which are in agreement with the Fund's books of account and we have obtained all the information and explanations which, to the best of our knowedge and belief, were necessary for the purposes of our audit. Proper books of account have been kept by the Fund, so far as appears from our examination thereof.

In our opinion and to the best of our information and according to the explanations given to us, the said Accounts give the information required by the Companies Act, 1948, in the manner required and present a true and fair view of the state of affairs as at 31st December, 1955, and of the excess of Income over Expenditure for the year.

19, Coleman Street, London, E.C.2. (Signed) TURQUAND, YOUNGS & CO.,

THE PALESTINE EXPLORATION FUND

RECEIPTS AND PAYMENTS ACCOUNT FOR THE YEAR ENDED 31st DECEMBER, 1955

1954	Receipts.	£ s. d.	£ s. d.
£			
	Balance—31st December, 1954:		
605	At Bank and in Hand	207 14 9	
183	Cash on Deposit	25 15 8	
788			233 10 5
502	Subscriptions and Donations, including local Societies	548 11 11	
67	Subscriptions in Advance	121 0 2	
73	Refund of Income Tax on Covenanted Subscriptions	72 5 3	
197	Sales of Publications	458 19 10	
675	Rents	843 15 0	
	Dividends Received—Gross:		
	On £374 12s. 0d. 3½ per cent. Conversion Loan (£ s. d.: 13 2 2)		
	On £300 3 per cent. Defence Bonds (9 0 0)		
	On £100 2½ per cent. Defence Bonds (2 10 0)		
	On £50 3 per cent. Savings Bonds 1955–65 (1 10 0)		
	On £264 1s. 8d. 2½ per cent. Savings Bonds 1964–67 (6 12 0)		
		32 14 2	
	Premium on Conversion of 3 per cent. Defence Bonds into 3½ per cent. Defence Bonds (3 0 0)		
33		35 14 2	
3	Interest received on Deposit Account	12 6	
—	Donations towards cost of publishing *Samaria*, Volume III	357 8 10	
			2,438 7 8
£2,338			£2,671 18 1

1954	Payments.	£ s. d.	£ s. d.
£			
510	Printing—Quarterly	753 9 6	
443	Annual 1953	—	
59	Miscellaneous	—	
1,012		753 9 6	
50	*Less:* Contribution by British School of Archaeology in Jerusalem	60 0 0	
962			693 9 6
41	Library and Librarian		7 6 9
605	Salaries, Wages and Pensions		605 4 0
58	Repairs		31 0 2
26	Insurance		22 1 11
127	Rates		56 0 4
44	Lighting and Heating		49 1 1
47	Postage and Telephone		53 1 5
134	Stationery and Sundries		105 11 5
10	Audit Fee		10 10 0
50	Contribution to Exploration by British School of Archaeology in Jerusalem		—
2,104			1,633 6 7
	Balance, 31st December, 1955:		
208	At Bank and in Hand	1,012 3 4	
26	Cash on Deposit	26 8 2	
			1,038 11 6
£2,338			£2,671 18 1

THE PALESTINE EXPLORATION FUND

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 31st DECEMBER, 1955

1954 £		£ s. d.	£ s. d.
	Payments for the year as per Receipts and Payments Account		1,633 6 7
	Less: Creditors at 31st December, 1954	302 8 5	
	Expenditure to be recovered from British School of Archaeology	17 0 4	
			319 8 9
1,325			1,313 17 10
4	*Add:* Depreciation on Typewriter ..	3 10 0	
302	Creditors at 31st December, 1955	396 8 4	
			399 18 4
1,631			1,713 16 2
5	Balance, being Excess of Income over Expenditure		194 8 9
	NOTE.—£100 has been included in respect of remuneration to a member of the Executive Committee.		
£1,636			£1,908 4 11

1954 £		£ s. d.	£ s. d.
	Receipts for year as per Receipts and Payments Account		2,438 7 8
	Less: Subscriptions in arrear at 31st December, 1954	9 11 9	
	Rent in arrear at 31st December, 1954	87 10 0	
	Rent in advance 31st December, 1955	62 10 0	
	Subscriptions in advance ..	121 0 2	
	Donations toward cost of Publication in 1956	357 8 10	
			638 0 9
1,412			1,800 6 11
	Add: Subscriptions in arrear at 31st December, 1955	9 18 0	
10	Subscriptions received in advance at 31st December, 1954 ..	66 15 0	
127	Rent in arrear at 31st December, 1955	31 5 0	
87			107 18 0
£1,636			£1,908 4 11

NOTE.—No depreciation for the year has been provided on Alterations and Improvements to No. 2, Hinde Street, or on Museum, Library and Storeroom, Fixtures, Fittings and Furniture, etc.

(Signed) GERALD M. FITZGERALD, Chairman,
OLIVER H. THOMPSON, Hon. Treasurer.

10th April, 1956.

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